

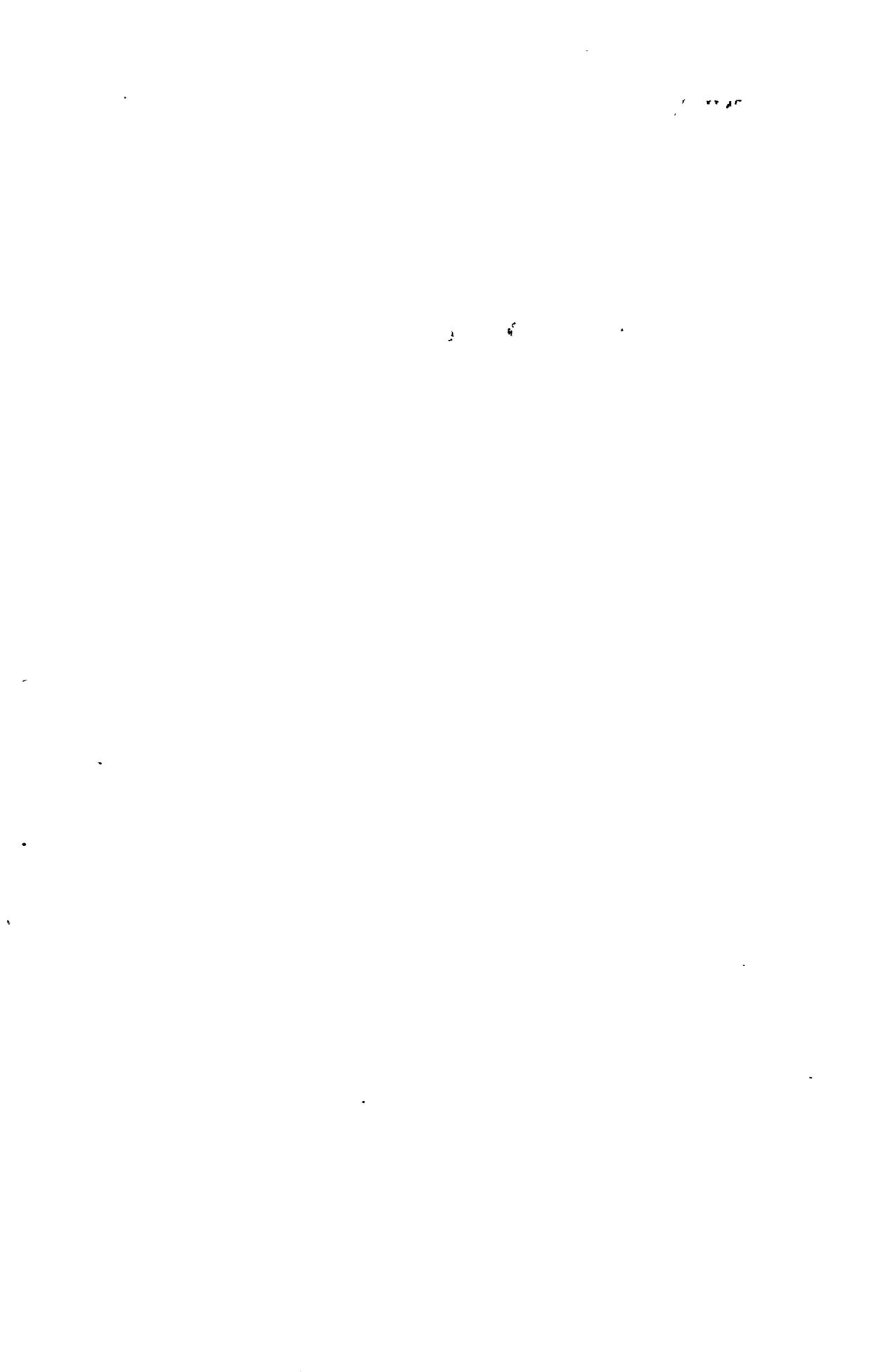
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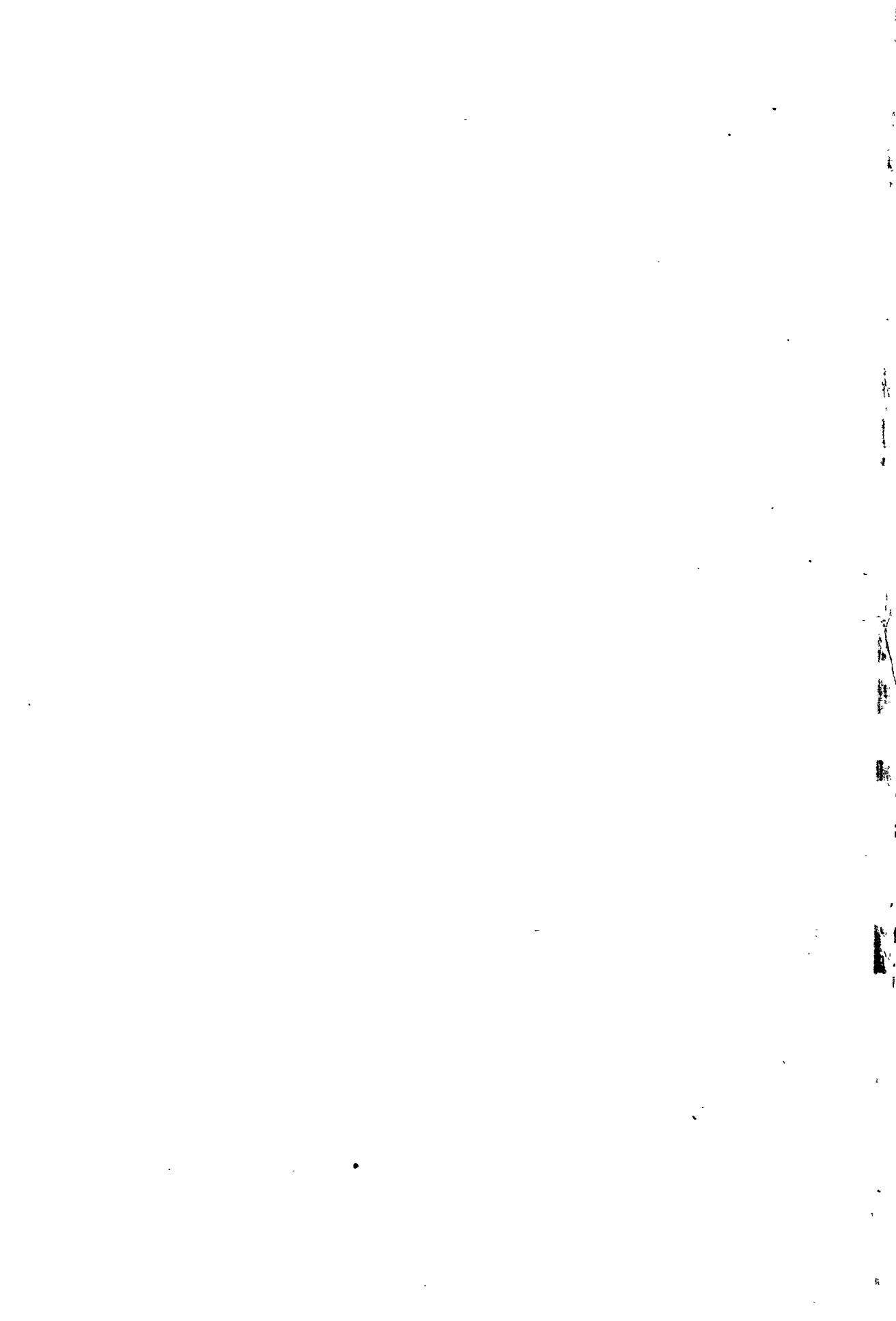
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FIGURE OF KUR-LIL (?), EL-'OBEID, 1919. (Brit. Mus.)

MAN

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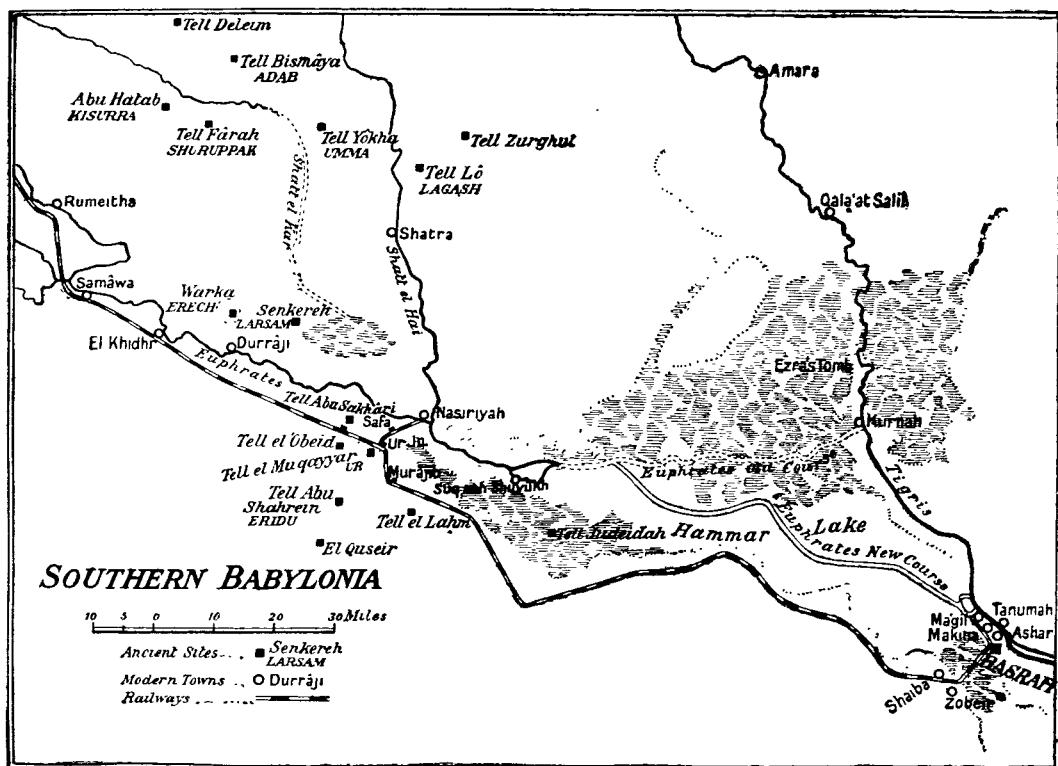
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Mesopotamia: Archaeology.

Hall.

The Excavations of 1919 at Ur, el-'Obeid, and Eridu, and the History of Early Babylonia (Brussels Conference, 1923). By H. R. Hall, M.A.

[The following paper was read at the Brussels Historical Conference last year, and is now published with additions, to bring it up to date.]



From "Proc. Soc. Ant.", Dec., 1919. By kind Permission of the Council of the Society of Antiquaries.

FIG. 1.

In the *Journal of Egyptian Archaeology* I recently published an account of the excavations carried on by me for the British Museum at Tell el-'Obeid, near

Ur-Kasdim, in southern Babylonia, in 1919, with some comparisons of the work and its results with Egyptian discoveries, especially those of Hierakonpolis. In this article I have cursorily alluded to my other

work at Ur itself (Tell el-Muqayyar) and at Tell Abu Shahrein, the site of the ancient Eridu. As, however, the original preliminary publication of the excavations of 1919 in the *Proceedings of the Society of Antiquaries* (December, 1919, p. 22 ff) had proved difficult of access to many scholars and students, especially those abroad. I decided to supplement the account of Tell el-Obeid and its Egyptian connections by a second article in the

FIG. 2.
THE TEMPLE-TOWER OF UR, FROM THE NORTH-WEST, IN 1919.

Journal of Egyptian Archaeology, which has now appeared, and described my finds at Ur and Eridu, with illustrations, as before. The two articles in the *Journ. Eg. Arch.* will act as a supplementary preliminary publication of the whole work of 1919. And as Mr. C. L.

Woolley has published in the *Antiquaries' Journal* a preliminary account of the Museum's work of 1922-3 at Ur, directed by him, which followed mine after a fallow interval of three years, and is just publishing his account of the work of 1923-4 in the same journal, students will be more easily enabled to understand the recent developments of my preceding work than if they had to refer back to the Society of Antiquaries' *Proceedings* of three years ago; they now have the whole evidence up to date under their hand, so to speak.

Mr. Campbell Thompson did a little work at Ur in 1918, from which mine developed, as Mr. Woolley's has developed from mine. We all follow in the footsteps of J. E. Taylor, who dug at Ur for the British Museum so long ago as 1854. Thompson's main work was not at Ur, but at Eridu, where in 1918 he followed Taylor with a most interesting and important investigation (to which I shall have occasion to refer very often), the results of which have been fully published by him in



FIG. 3.—AN EXCAVATED SUMERIAN HOUSE AT SHAHREIN, 1919.

Archæologia, the chief organ of the Society of Antiquaries, for 1920 (vol. lxx., pl. 101 ff.). My own work at Eridu was of much less account than his, but was productive of interesting results, also including houses of about 2500 B.C. and the stone walls of ancient Eridu. Tell el-Obeid was a new discovery of my own. It has produced, during my excavations of 1919 and Mr. Woolley's of 1923-4, very important monuments of the early Sumerian period and in order that these and the general results of the work might be better known abroad, and that the historical importance of the finds of 1918 and 1919 might be appreciated by Continental scholars, I had the honour to lay this paper before the Brussels Historical Congress.

For Mr. Thompson's work I must refer my readers to the volume of *Archæologia* in which it is published. I will only inform those who have not seen his publication that he made a very thorough investigation of the Shahrein mounds which conceal the remains of ancient Eridu, and that the pits and trenches that he dug systematically in order to probe their secrets literally to the bottom, produced most valuable evidence as to the stratification and as to the date of various types of antiquities found. A peculiar characteristic of Shahrein is the "fan" of detritus that extends around the mounds, and has carried with it out on to the desert thousands of objects belonging to the lower strata of the mounds themselves, as has been ascertained by means of Thompson's pits. The loose sandy mounds are torn every winter by rain-floods, which for centuries have at Shahrein scooped out their way down from the *ziggurat*, making deep wadis, and carrying with them remains of all ages, which now lie out on the surface around the mounds, mingled with the similar remains from early cemeteries and settlements outside. At el-Obeid the original mound was so small that comparatively few of the similar outside surface finds there can have been washed out of it, and Mr. Woolley has laid bare the settlement and cemetery to which they belong, with graves of an older and a later period. Thompson's work has resulted in a rough general classification of the objects found, whether *in situ* or on the surface at Shahrein, and Woolley by his excavation of the cemetery has in succession to me confirmed at el-Obeid the results obtained at Shahrein. At both places the most important find has been that of pottery with painted geometric and stylized naturalistic designs in black (Figs. 4, 5). Very few complete pots were found: only innumerable fragments. The ware is usually handmade, but shows signs of the incipient wheel—the "slow-wheel" method. It is usually very fiercely fired, hard, almost vitrified: greenish-drab in colour, with designs in bright black. On less highly fired fragments (which are less common) the designs often appear in reddish, rarely in quite red, pigment. The more highly vitrified the pottery, the blacker



FIG. 4.—PREHISTORIC PAINTED POTTERY, EL-'OBEID.

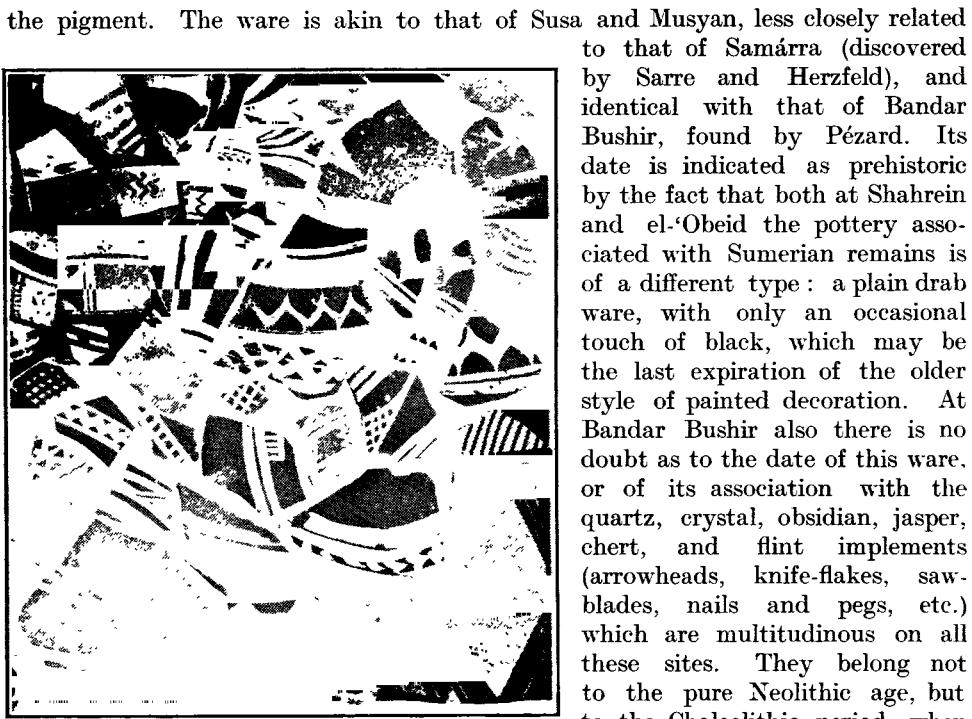


FIG. 5.—PREHISTORIC PAINTED POTTERY; EL-'OBEID.

in. A very curious object, which is common is a sickle of hard vitrified pottery, which may have been votive, but is regarded by Thompson as intended for actual use. On the other hand, the serrated blade of flint, which is common at el-'Obeid is rare at Shahrein.

Thompson ascribes to the culture of the prehistoric pottery a "pre-Sumerian" date, and believes it to be the relic of an Elamite population which spread over the fens-lands of Babylonia at an early time, and was afterwards either expelled or overlaid by the Sumerians.

Personally, however, I do not see that an Elamite population need be postulated at all. The older culture (or, more accurately, the older fashion or style of pottery and implement-making) may be equally Sumerian with the culture of the time of Ur-Ninâ: the race need not have

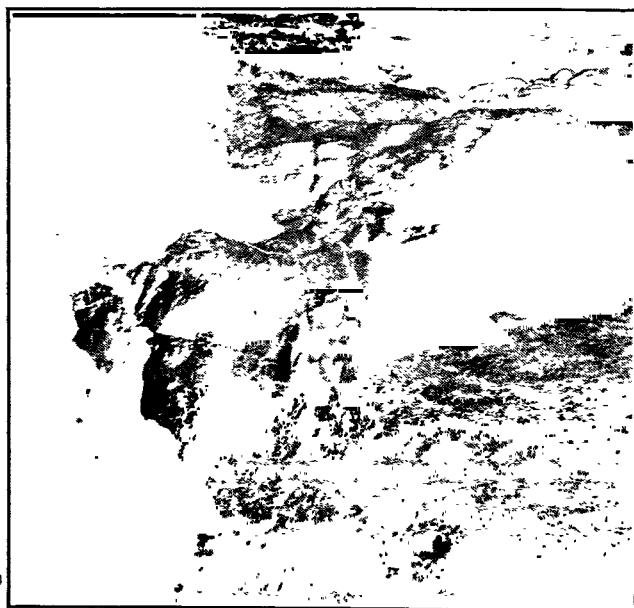


FIG. 6.—A COPPER BULL AND TESSELATED PILLARS, EL-'OBEID; DISCOVERED IN 1919.

altered. The Elamite culture was fundamentally the same as the Sumerian, although their languages differed. It seems to me unnecessary yet to postulate either an Elamite conquest of Babylonia in pre-Sumerian days or a primitive Sumerian conquest of Elam, till we have more evidence. Culture from a common source is extremely likely; but if the common source is to be assigned definitely, it seems to me that the great river valley has quite as good a claim to be regarded as the mother of civilization in those parts as the hills and plains of Elam. But, until we know more definitely, I should deprecate speaking of a pre-Sumerian Elamite population in Babylonia, and would call the whole primitive population and culture of Babylonia and Elam by the common name of Sumero-Elamite. Into the question of possible northern origins for the joint culture, advocated by M. Rostovzev, I will not enter here. Much further study and cogitation is required before we can definitely accept or reject it. My idea of a possible eastern origin in pre-Aryan India (*Ancient History of the Near East*, p. 173-4) has lately received unexpected support in the astonishing finds at Mohenjo Daro in Sind and Harappa in the Panjab, the Sumerian connections of which, not mentioned by Sir John Marshall in his publication in the *Illustrated London News* of September 20, 1924, were at once noted by Sayce and other Assyriologists and were exemplified by Sidney Smith and Gadd in the *Illustrated London News* of October 4.

My work at el-Obeid resulted also in a very interesting discovery. At one end of a small pre-Sargonic building of burnt brick (which I thought to be a temple tower and Andrae thought was a fort, but has since been found by Woolley and Gadd to be certainly a temple of the goddess Nin-khursag), I found, beneath the remains of a crude brick platform which may be assigned to about 2250 B.C., a *favissa* full of very important works of Sumerian art, then obviously of the Ur-Nina period (*circa* 3000 B.C.) or earlier, which are now, since the further discoveries of 1924, known to belong to the First Dynasty of Ur (between 3600 and 3200 B.C.). There were the foreparts of four life-size lions with heads of copper, filled with bitumen and clay, with eyes of red jasper, white shell, and blue schist fastened into the bitumen with copper wire, teeth of shell, and tongues of red jasper. There were no hinder parts, and the torsoes looked as if they had been the supports of a throne, though it is more probable that they were originally half imbedded in a wall. With these were two smaller lion-heads, and two panther heads of the same technique but with the eyes merely chased, not inlaid in stone and shell.

There were two figures of bulls (Fig. 6) of copper (one of which dissolved into a green powder of oxidization almost immediately after discovery), and the other was only recovered in fragments, the head of a third, and a golden horn belonging

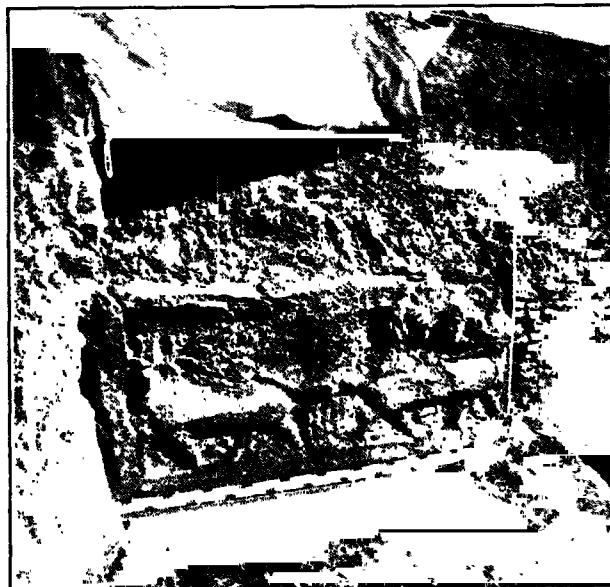


FIG. 7.—THE IMIGIG RELIEF, EL-'OBEID; 1919.

to one of them. Mr. Woolley has this year succeeded in bringing back two practically perfect figures of the same kind.

There were also birds' heads, and, above all, the remains of a great relief. 2.44 m. long by 1.07 m. high, of copper, representing Imgig, the lion-headed eagle of Lagash, holding two stags by their tails: an "antithetical" group well known in Babylonian antiquity (Fig. 7). It possibly denotes the sovereignty of Lagash over Ur and its territory somewhere about the time of Ur-Nina. The statue of Entemena, found by Woolley last year at Ur, may indicate the same thing.

The copper of all these monuments is in a very bad condition, especially that of the Imgig relief, which, however, it is hoped to restore with some success. The date of these objects is fixed, apart from their style, by a very archaic inscription on the fragment of a limestone statue found with them, which commemorates Kur-Lil, doorkeeper of the temple of Erech, who dedicated the figure in the temple of Damkina (identical with that of Ninkhursag?).

An almost perfect figure of a squatting man of the same type, in trachyte (Plate A), was found with it, and probably is a portrait of the same man. An interesting find (repeated this year by Mr. Woolley) was four pillars of tesselated work, a mosaic of black, white and red triangles, the white being nacre or mother-of-pearl. This has been paralleled in vase-stands at Musyan in Elam (Louvre).

El-Obeid lies about 6 km. west of Ur proper, and 14 km. north of Shahrein. It was no doubt a "suburb" of Ur. The name is that given to it by the *fellahin* of the Rif, who also called it Tell el-'Abd ("the mound of the slave"), while the desert Arabs of the *Col* call it Tell el-Ma'abed ("the mound of the



FIG. 8.—A POTTERY COFFIN AND BURIAL AT UR.

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Of the results of the work at Ur itself it remains only to say that the S.E. face of the ziggurrat (Fig. 1) was cleared and a building ("B") found which may either be *Ê-kharsag* ("the house of the mountain"), a palace of Shulgi (as the name of the king formerly known as Dungi is now written) or part of the temple of Nannar, the Moon-god, erected by his predecessor Ur-Nammu (who used to be called Ur-Engur). The temenos wall was discovered and partly excavated. This excavation has been continued by Messrs. Woolley and Newton, who have found in it gates of Bur-Sin I. and of Cyrus and have traced its full length round the ziggurrat, showing that my building "B" lay within the temple precincts, so that even if it was *Ê-kharsag*, it can also be regarded as part of the temple. A foundation-tablet of *Ê-makh*, the temple of the goddess Ninsun, dedicated by Ur-Nammu, was found. Tombs (with

clay coffins containing the skeleton, always in the crouched position (Fig. 8) and streets of the town of Ur were excavated, as also at Eridu (Fig. 3); and clay cuneiform tablets were found which include some of the earliest prognosticatory and medical texts known (of the time of the 3rd Dynasty of Ur, c. 2200 B.C.) and some minutely inscribed legal documents (wills and sales of legacies) of the seventh century B.C. The full publication by Mr. Woolley and myself of the results of the excavations of 1923 and 1924 as well as 1919 will follow in due course.

H. R. HALL.

America, Central: Chronology.

Long.

The Bowditch and Morley Correlations of Maya Chronology.

By Richard C. E. Long, B.A.

2

In a paper in the *J.R.A.I.* for 1923 and in several papers in *MAN* I have furnished proofs of the Bowditch correlation of Maya and Christian chronology. As the evidence in these papers was of a calendrical nature, I propose in the present paper to deal with evidence of a historical nature and also with some objections which have been made.

Now, according to the Morley correlation, the Katun 8 Ahau, in which occurred the fall of Chichen Itza, when its governor, Chac Xib Chac, was driven out by Hunnac Ceel, the governor of Mayapan, was equivalent to 11-12-0-0-0 in the Long Count, and the Katun 8 Ahau in which occurred the fall of Mayapan was equivalent to 12-5-0-0-0. In the Bowditch correlation the former of these Katuns was equal to 12-5-0-0-0 and the latter to 12-18-0-0-0. There are at Chichen Itza a large number of buildings of Nahua type, and at Uxmal there is that most distinctively Nahua monument, a ball court. On this building Dr. Morley found the date 11-15-16-12-14. His reading of the inscription must be accepted and it proves that there was Nahua influence in Yucatan before 12-5-0-0-0. The same result follows, though with less certainty, from his reading of 11-12-8-13-4 as the date of the lintel in the structure east of the *casa principal* at Chichen Itza. Here, if his ingenious suggestion of the day, 6 Kan, being used as a year-bearer is correct, as I believe it is, it also shows Nahua influence, since the use of year-bearers to distinguish the years seems a purely Nahua practice, unknown in the early Maya. Accepting, then, the evidence of these monuments that there was Nahua influence in Yucatan before 12-5-0-0-0, it follows that if it can be shown that there was Nahua influence there before the fall of Chichen Itza, it is very strong evidence in favour of the Bowditch correlation, while, if such influence only commenced after the fall of Chichen Itza, it is equally strong evidence in favour of the Morley correlation. The question may be considered a crucial one between the two correlations. Now the only information in the Books of Chilan Balam is the names of the "seven men of Mayapan" who were the allies of the victorious Hunnac Ceel when he brought about the fall of Chichen Itza. Most of these names are Nahua and this shows that there were Nahua in Yucatan then and presumably afterwards, as they were on the winning side, but it does not prove that there were no Nahua in the country before. Landa, referring to the time between the fall of Chichen Itza and the fall of Mayapan, says that the rulers of Mayapan maintained their power by the help of Mexican soldiers. From this Dr. Morley concludes that Chichen Itza was handed over to these mercenary soldiers by the ruler of Mayapan, and that it was these mercenaries who erected the buildings at Chichen Itza which show Nahua influence. I cannot but think that this conclusion is unsatisfactory. There is no mention in any of the sources of Chichen Itza being so handed over, and it seems contrary to all probability that a ruler who kept himself in power against his subjects' will by the aid of foreign mercenaries would station these troops in any other place than his own capital Mayapan, where they could protect him, and, besides, there is Landa's direct statement that the ruler of Mayapan "negotiated with the garrisons of troops which the

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" Mexican kings maintained in Tabasco and Xicalango to hand over to them the guard of the capital. In this way he brought Mexicans to Mayapan." It is also highly improbable that great buildings such as those of Chichen Itza would be erected in a conquered city converted into a mere camp of foreign mercenaries, and not in Mayapan the capital. The Books of Chilan Balam show that this period was one of wars, unfavourable to the erection of great buildings. The monuments of Nahua type at Chichen Itza are some of the greatest in Yucatan and have all the appearance of having been built while the city was the seat of powerful rulers, so it seems probable that they date from the time, prior to the fall of Chichen Itza, when the three allied cities of Uxmal, Mayapan and Chichen Itza jointly ruled the whole country. If we admit Nahua influence at this time in Yucatan it explains the ball court at Uxmal, which, on the Morley theory, is unaccounted for. The evidence points to these buildings being the work of an earlier branch of the Nahua, distinct from the later Nahua described by Landa as Mexicans, that is Aztecs, while the character of the Nahua remains at Chichen Itza is not Aztec, but rather of an earlier Nahua type.

Mr. R. L. Roys, in two papers in the *American Anthropologist*, namely, "A New Maya Historical Narrative" (in 1922) and "The Ritual of the Chiefs in Yucatan" (in 1923), inclines to the opinion that there were earlier Nahua in Yucatan than the mercenaries mentioned by Landa. He shows that the Tutul Xiu were probably of Mexican origin, and he attributes the ball court at Uxmal to such earlier influence. But I think that the matter can be carried much farther than he has done, and that a much stronger—in fact, a conclusive—argument can be drawn from the former of his papers. He shows from a passage in the Chilan Balam of Chumayel that Hunnac Ceel, the conqueror of Chichen Itza, had himself been previously thrown into the Cenote of Sacrifice there, and that, as he had survived for the required time, he had been taken out and worshipped according to custom. Now, that the custom of throwing victims into the Cenote was a purely Nahua rite, not a Maya one, can also be proved by what Mr. Roys translates in his paper. We find in it "And then the tribute of Holtun Zuiva came." Mr. Roys explains Holtun Zuiva as "the cave of Zuiva," and says that it appears to symbolise the Nahua origin of the ruling powers at the time. It is generally agreed that the word "Zuiva" or "Suiua" is connected with legends of Nahua origin wherever it occurs. A little farther on is the passage "Then began the arrival of those who were thrown into the well." These passages, therefore, show that only after the Nahua rule was established was the custom of throwing victims into the Cenote introduced. If, then, this rite was only introduced after the Nahua became powerful at Chichen Itza, it follows that the fact of Hunnac Ceel himself having been thrown into the Cenote is a proof that this rite, and therefore the Nahua rule there, were in existence *before* the fall of Chichen Itza, and it is a conclusive disproof of Dr. Morley's contention.

It follows from this that the great ball courts at Uxmal and Chichen Itza, and the other great Nahua buildings at the latter site, all which were contemporaneous with the practice of throwing victims into the Cenote of Sacrifice, must date from the period of the Triple Alliance. But if that be so, then the date 11-15-16-12-14 on the ball court at Uxmal is consistent with the Bowditch correlation, but develops an impossibility on the Morley one, because this date falls after 11-12-0-0-0 which on the Morley system is the date of the fall of Chichen Itza, that is, it would be after the rule of the early Nahua came to an end. I think, therefore, that the foregoing may be fairly claimed as a convincing proof that the Bowditch system is correct.

Let us now examine the dates in Yucatan deciphered by Dr. Morley in his *Inscriptions at Copan*. They may be divided into two sets: (1) those fixed in the Long Count, and (2) those fixed only in the Katun Count. As the latter can occur every thirteen katuns and satisfy the given conditions, their initial series values will depend upon (a) at what points in the Christian Era they are supposed to fall, and (b) what correlation of the Long Count with the Christian Era is adopted.

Table II is based on the assumption that these dates in the Katun Count fall on the same dates in the Christian Era as Dr. Morley puts them at (that is approximately, subject to a minor correction of about fourteen years which need not be entered into here) and the initial series values of each are given both according to the Bowditch and the Morley correlations.

TABLE I.—DATES FIXED IN THE LONG COUNT.

Chichen Itza. Structure east of the <i>casa principal</i>	- - - -	11-12- 8-13- 4
Uxmal. Monjas	- - - -	11-12-17-11- 1
Uxmal. Ball Court	- - - -	11-15-16-12-14
Chichen Itza. High Priest's Grave	- - - -	11-19-11- 0- 0

TABLE II.—DATES IN THE KATUN COUNT.

	Morley Correlation.	Bowditch Correlation.
Ichmul	- - - - 11-19- 0- 0- 0	- - - 12-12- 0- 0- 0
Loltun	- - - - 12- 1- 0- 0- 0	- - - 12-14- 0- 0- 0
Mayapan	- - - - 12- 4- 0- 0- 0	- - - 12-17- 0- 0- 0

Now, on the Morley correlation the dates in the two tables overlap, so that Ichmul is earlier than the last date at Chichen Itza and Loltun is only about 29 years later than it, while on the Bowditch correlation not only does each set fall in a different cycle, but the distance from the latest in one to the earliest in the other is over 240 years. I think that it is a most significant fact, and is a further confirmation of the Bowditch correlation, that on it there is such a wide interval of time between these sets of dates, owing to the difference of the method of dating between them. On this correlation the Katun Count is shown to be a late practice, only used during the closing period of the New Empire after the fall of Chichen Itza, in fact long after it and near the time of the fall of Mayapan, while the earlier inscriptions which still show a relation to the Long Count, though expressed in very anomalous ways, all date from the time of the Triple Alliance or slightly earlier. This question of the method of expressing dates is very important and has been ably dealt with by Dr. Morley, as regards the Old Empire, in his *Inscriptions at Copan*, in which he traces the gradual change from the use at first of initial series ending on casual dates, then to those ending hotuns, then to the use of several dates on a monument along with the hotun endings, and finally to the disuse of initial series and the substitution of period ending and calendar round dates.

In this connection, I venture a small criticism of a great work, namely, that it would have been desirable if he had restricted the term "period ending" to those cases where the date is expressed in the inscription to be a period ending, either by an ending sign or by giving the number of periods which it closed. He not only calls these period endings (which I submit are the only ones that should be so called) but he also applies the term to calendar round dates which are inferred to terminate periods because there are periods ending on those dates. The inference is no doubt perfectly justifiable, but still it is an inference only, and when the Maya did not express it as a period ending we should not call it so. Also he applies the term "period ending" to secondary series dates when they fall on dates which are the ends of periods, but have not the above defined marks of a period ending. The same objection applies here, because though we can prove that they do end periods, yet the Maya did not express them as such. The distinction was kept clear by Bowditch, and by Dr. Morley himself in his "Introduction to the Study of the Maya Hieroglyphs," and it should be maintained in the interest of accuracy and clear definition.

Coming now to the New Empire we again find a slight change of method in the Lintel in the Temple of the Initial Series at Chichen Itza. Here there is an initial

series ending on a casual date. There is no secondary series, but a period ending follows which differs from the usual practice of the Old Empire in having no month date, so that, like the Katun Count, it can recur every thirteen katuns. Stela I at Tulum may be a similar case, but as it is broken there may have been a month date on it. Both these are in Cycle 10, but when we come to Cycle 11 we have the monuments in Table I above, which show methods of fixing dates differing from one another and rather bewildering to a student used to the Old Empire inscriptions. The general conclusion follows that the method of dating changed at different stages of Maya history, just as the style of sculpture did, and this confirms the view expressed above.

On another historical point, Dr. Morley objects to the Joyce correlation on the ground that it implies that there were earlier Maya at Chichen Itza than those whose history the Chilan Balam of Mani relates, and he quotes Mercer's "*Hill Caves of Yucatan*" to show that the Maya were the first inhabitants of the peninsula. No doubt they were, but this does not prove that no earlier branch of the Maya than those mentioned had settled there. It is most probable that, like the Nahua in Mexico, there were many tribes of the same race who came into the country at different times. Dr. Morley himself sees no difficulty in this in other cases. In his paper "The Rise and Fall of Maya Civilisation" in the Nineteenth Americanists' Congress *Proceedings*, he says, on page 146, speaking of the migration to Chakanputun, "Doubtless Maya from the Old Empire were still 'migrating northward into Yucatan and opening new localities.'" Then, if the migration took place in this way, what is the objection to the same having taken place at Chichen Itza?

It appears then that all the historical evidence supports the Bowditch, not the Morley, correlation. As to the calendrical side of the argument, I have gone into it in detail in previous papers, but it may be well here to mention some of the principal points. The Bowditch correlation gives a result which is accurate to the day; it makes the most important date of the Dresden Codex fall on the summer solstice; it explains the discrepancy between the correlation derived from the date given for the death of the water-bringer and that derived from most other dates by showing that this is exactly accounted for by the number of intercalary days required to correct the calendar at that point; it gives a probable explanation of the invention of the Long Count and also of the later invention of the count by higher periods than the cycle; it explains the date at Santa Rita; and it gives a probable explanation of the "setting in order of Pop." Also in my paper, "A Link between the Earlier and Later Maya Chronologies" (MAN, 1924, 66) a connection is shown between a date in the New Empire and the Long Count. The objection has been made to me by a well-known authority on the Maya that this explanation of mine was untenable as it requires "uuclahunpiz katun" to mean "17 katuns" and that the Maya for "17 katuns" would be "uuclahunpiz" "katunob," with the plural suffix "ob." But the only place where katuns appear to be counted in the Books of Chilan Balam (Brinton, "*Maya Chronicles*," p. 95) has "cante (bin ti) katun," "4 katuns," thus showing no suffix "ob." Further, in Tozzer's "*Maya Grammar*," though it appears that the words "uinic," "man," and "na," "house," have the plurals "uincob" and "naob," yet on page 103 he gives "oxtul uinic," "three men," and "oxpel na," "three houses" (I use the usual Maya spelling, not Tozzer's), again showing no plural suffix. So my explanation does not seem open to this linguistic objection.

Now, I think that the argument from the intercalary days and from the date falling on the summer solstice are themselves proofs amounting to demonstration of the truth of this correlation even without the other confirmations of it, and this paper shows that there is just as strong a demonstration on the historical side, so

that, although it may appear daring to challenge the views of so great an authority as Dr. Morley, yet it seems to me that the correlation problem is solved.

RICHARD C. E. LONG.

Archæology.

Burkitt.

Archæological Notes. *By Miles C. Burkitt.*

The following are a few notes that may be of interest to readers of MAN.

3

I. The problem of how far Solutrean implements occur in England has long exercised prehistorians. The laurel leaf is by no means a typical tool, unless it is found stratigraphically in Upper Palæolithic deposits or associated with quaternary fauna. Single tools, especially when they are not massive, easily get displaced, sometimes to considerable depths, and a laurel leaf found isolated may be of almost any date from Solutrean to Bronze Age times. However, in the case of the Upper Solutrean single-shouldered point showing pressure thinning-flaking at the tip on the under surface, this is, I fancy, a more typical "fossil" and an example of such a specimen has turned up in our collections in the Cambridge Museum, from Lakenheath (Suffolk). Fig. 1a shows a typical French Upper Solutrean specimen, while Fig. 1b is the Lakenheath example. Except for size, they are extraordinarily similar. Of course, it would be unwise to presume from the rare examples of Solutrean types in England (Kent's Cavern, etc.) that the Solutrean race really inhabited Great Britain. It would seem safer in the present state of our knowledge to assume that the steadily developing Upper Palæolithic cultures of Neoanthropic man in England were merely influenced to a greater or less extent by the Solutrean culture of western France—a land more favoured from the climatic point of view. All that has been said of the Solutrean probably applies equally in the case of the Magdalenian cultures.

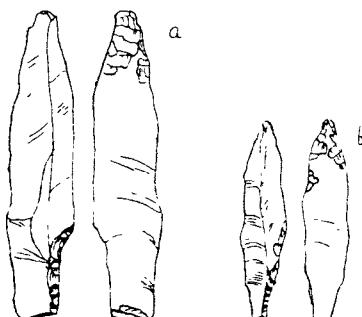


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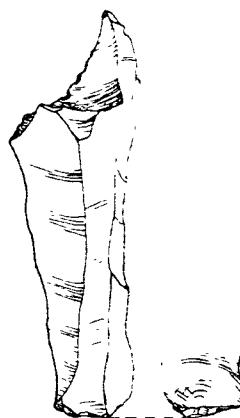


FIG. 2.

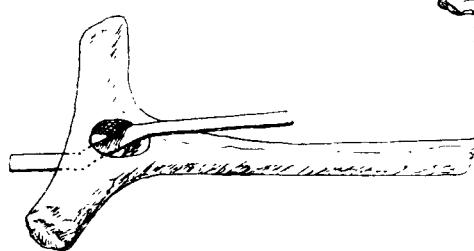


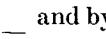
FIG. 3.

II. Having had occasion to examine certain caves in the Yealmpton district near Plymouth, I came across a deposit which may prove to be a new Upper

Palæolithic site. It is hoped to continue digging next year. Fig. 2 is a flake with a deep white lustrous patina, and it can be matched in every respect from the Upper Palæolithic finds at Burrington Combe, etc. The ancient entrance to the cave in question is blocked up, but the back of it has been cut into by a now disused quarry. From the back of the cave towards the entrance a deposit of cinders some 6 ins. thick was found at a depth of about a foot. The dig being on the precipitous side of the quarry it was not possible in the short time available to do more than rather drastically cut back a few feet to allow for safety and elbow room. Nor has the actual depth of the deposit, or the existence or not of other layers of cinders, yet been determined; but should, as is probable from the meagre finds, the cave have been the home of an Upper Palæolithic hunter, it will be interesting as being in a new area.

The only information available on the Yealmpton Caves, and it refers almost exclusively to another group a mile away, was collected by W. Pengelly and published in the transactions of the Devonshire Association for the Advancement of Science, Literature and Art in 1870. No trace of man was found, but a fairly rich quaternary fauna, including elephant, rhinoceros, etc., was discovered. Up to the present the diggings in the new cave have shown nothing of importance in the way of fauna, not enough work having been done, except in the superficial layer above the hearth, where, however, the loose sandy soil has been much disturbed by rabbits and the like.

III. Prehistorians interested in Upper Palæolithic art should be on the look-out for descriptions of a new find of a tiny statuette representing a mammoth, made apparently of a sort of sandstone, reddish in colour, from the Loess on the borders of Moravia and Austria. The figurine is at present at the National Museum at Vienna, where it is being studied and described. In this connection one might also draw attention to the new "Venus" in mammoth ivory, from Kostienki in the Lower Don region, about half-way from Moscow to Rostov. For further description of this, *L'Anthropologie*, Tome XXIV, No. 3-4, should be consulted. The age is certainly Upper Palæolithic, but it comes from the eastern "province," where the Upper Palæolithic cultures do not quite correspond with those of the West. All these new finds of Palæolithic art are very interesting and go to increase our knowledge of the wonderful folk who made them.

IV. Various uses have been suggested for the so-called *bâtons de commandement*. Thus it has been stated that they were used as arrow-straighteners, as sceptres, and even as brooches. Personally, I should hesitate to deny any of these suggestions, believing, as I do, that they were not designed for one purpose only, and it does not follow that the first examples, dating from Middle Aurignacian times and found at Sergeac, were used for exactly the same purpose as the fine, delicate, decorated examples of Upper Magdalenian times. Dr. Porsild, who has lived long among the Eskimos in Greenland, has lately suggested to me a still further use for these objects. Pliable thongs are required by the Eskimos and were probably also required for haulage and other purposes by Upper Palæolithic man. It appears that in order to render strips cut from the raw hide pliable they are threaded through just such a tool as our *bâton de commandement*, which is then moved rapidly up and down with a stropping motion, the thong being meanwhile held taut. It is significant that the marks of wear in the holes of several prehistoric examples occur exactly where they would do had the tools been used for such a purpose. It can clearly be seen from Fig. 3 that successive portions of the thong would thus be continually bent into the form  and by this means the whole strip would very rapidly become soft and pliable.

MILES C. BURKITT.

REVIEWS.

Pottery.

Frankfort.

Studies in Early Pottery of the Near East: 1. Mesopotamia, Syria, and Egypt and their Earliest Interrelations. By H. Frankfort, M.A. 146 pp., 13 plates. 28 x 19 cm. Occasional paper No. 6. Royal Anthropological Institute. 1924. Price 10s.

4

This essay is a most welcome gathering of the scattered material relating to early civilisation, discussed with ability and insight. Not only the published objects, but also much in museums and not exhibited, has been searched out and judged at first hand. The opening is on the evidence drawn from pottery and technical production. This is very careful and cautious, but it may give an impression that little can be proved. Yet practically the appearance of pottery of any age and place is so distinctive, and depends on so many different factors which are not likely ever to recur all together, that there is no hesitation in recognising the source, except for the roughest ware. What cannot be done is to draw conclusions from a single line of resemblance. Unfortunately, most people do not see what is really characteristic, and weary one with claiming that things are alike when to a trained eye there is no chance of connection.

In the origins of design the skeuomorphic and magical are noticed, but there is also a large element of the purely artistic pleasure in design which has no relation to these other sources. This is shown by the dozen different plants in the earliest paintings of Egypt. A strong emphasis is laid on the difference of young, natural, and varied styles as opposed to the old, formal, and perfunctory. In another question of principle the author seems to dread the imputation of recognising invaders as bringing in fresh culture. When we look at the oldest human figures—long haired and bald—fighting; at the Hierakonpolis tomb—red and black—fighting; at the Arak knife-handle with two races fighting and a pool of dead bodies; at the slate palettes with fighting and a field of slain; at the Second Dynasty statues with the record of 47,209 slain—it is a ludicrous infection of pacifism not to recognise that mankind has advanced by continually wiping out the less competent people. It is impossible to agree that "hostile invasion" is "only in exceptional cases."

To come to the real thing, there is a very careful study of the first Susian pottery, copied from leather work, and its spirited designs. This serves as a type analysis to point out that no other pottery is really like it or can be claimed to descend from it. The first civilisation seems to have been destroyed by drought, which made the site desolate for long, before the second civilisation came. That was linked with Anatolian features, and both of these passed away before the Sumerian invasion. These people are allowed to be probably of Dravidian connection, coming along the south of Persia up to the Tigris region. The older Mesopotamians used bow and arrows, hunted wild cattle, and wore fine linen; the Sumerians had no bow, brought domesticated cattle, but had only coarse, shaggy clothing. Their Indian linking has since been strongly confirmed by the new discovery of seals with carvings and signs in India closely like the Sumerian. The Anau civilisation does not enter into this period, as the earliest of it has copper.

In the questions of Egyptian connections this work was prepared just too soon to include the earliest stage now known, the Badari culture. The ground has been much more completely worked than in Asia, and the conclusions therefore hardly do more than collect what is already familiar. The dissimilarity of the earliest patterned pottery, white cross-lined, from the Susian ware, is pointed out. We now know that, while the painting is Algerian, the pottery and other objects have an ancestry in the Badarian (Caucasian ?) family. The later prehistoric wave is accepted as originating in the Red Sea mountains, and adopting the ship design after settling on the Nile.

The thoughtless observation that "Egypt is a tube which can only be entered at top and bottom," is repeated. Historically, this is untrue; the Westerners have flooded in across the desert a century ago, under Fatimites, Saites, and earlier, from the Oases; the Dynastic people came in through the Wady Hammamat. The inflow has been as serious at the sides as at the ends. The wavy-handled jars are identified with the Palestine forms, and supposed to have been imported with oil, and copied later in the cylindrical form in Egypt.

The North Syrian connection is well brought out; the bird vases in the late prehistoric age, and the foreign pottery of the First Dynasty, being traced to the north of the Lebanon.

The Mesopotamian influence shown by the serp-leopards, brick panelling, socket stones, decorated mace heads, pottery stands, and cylinder seals, is strongly and convincingly stated; and enforced by the collective appearance of all these relations together at one time, as proving an active immigration, pictured in the battle scenes on ivory and slate. The only possible road was by Hammamat, all other lines being barred by various details.

The Institute is to be congratulated on having added such a piece of work to its publications. Mr. Frankfort's further research on Mediterranean pottery will, we hope, be equally judicious and appear in the same manner.

FLINDERS PETRIE.

CORRESPONDENCE.

Egypt: Archæology.

Burchell.

To the Editor of MAN.

Objects from El Kab.

SIR.—With further reference to MAN, 1924, 28, relating to the El Kab candlesticks, I have received through the kindly courtesy of Professor Hoffmann-Krayer, of Basel, permission to reproduce an illustration appearing in the *Schweiz. Archiv f. Volkskunde*, Vol. XXII, pt. I. 5

This gives an example of one of the many Neolithic bark torches found in the Neolithic pile-dwellings at Burgäschisee.

Since this proves that Neolithic man of Switzerland made use of torches, may not we assume that the Egyptians of the IVth Dynasty were equally civilised?

Yours faithfully,
J. P. T. BURCHELL.

Psycho-analysis.

Hocart.

To the Editor of MAN.

Psycho-analysis and Anthropology.

SIR.—Dr. Ernest Jones, in his lecture on "Psycho-analysis and Anthropology," expounds Freud's doctrines that "dream thoughts" never contain a negative, so that a positive idea and its exact opposite are treated as identical. He thinks with Freud that the phenomenon is 6

common in the early stages of the oldest languages, and has left interesting traces even in modern languages, such as with the word "cleave," which means both to "adhere to" and "to separate." Now, if the lecturer had gone no further than his "Concise Oxford Dictionary," he would have found that we have here not one word but two, which have only become identical in sound in recent times, and are still represented by distinct words in German; even in modern English they are only identical in the present tense: the past of one is *cleaved*, of the other *clove* or *cleft*. In German the present tenses are distinct: *kleben* and *kleben*. The further back we trace these two words the wider apart we shall find them to be. The identity in sound of *cleave*, *clave*, and *cleare*, *clore*, is, therefore, purely accidental; it is not due to any such mental processes as Dr. Ernest Jones supposes, but to a series of sound changes which have been going on from prehistoric times and have resulted in an identity of sound. This is an exceedingly common occurrence, of which English has countless examples, e.g., *pale* (two words) and *pale*, *male* and *mail* (2), *faint* and *feint*, *knot* and *not*, etc., etc.

I may be told this is a small point, and that I am picking holes. It is not a small point, for it is a point of method: the method is all wrong, and from a wrong method we can only expect the truth by accident. Yet it is the invariable method of psychologists in historical matters. They do not go first to the historian and ask him exactly what has happened, and then seek to deduce the mental processes from the succession of forms; he evolves an explanation which accords with his psychological theories, then appeals to that explanation as a confirmation of his theories. This method has been applied not only to language but to customs in general. A picture of an Indian myth, for instance, is psycho-analysed as if it had no history behind it; the fact that Greek art, generations of Indian artists, then Mahomedan, then European influence have contributed to shape its artistic conventions is entirely ignored; and then the psychologist is surprised that the historian rejects his work and finally refuses even to read it. I have attempted in this letter to explain by means of a concrete example the reason why.

Yours faithfully,

A. M. HOCART.

Archæology.

Rose.

To the Editor of MAN.

Egypt and India.

SIR,—The testimony of Philostratos to an early connection between Egypt and India, mentioned by Mr. F. W. H. Migeod (MAN, 118, Oct., 1924) **7** would be interesting and important if its source were less unreliable. The "Life" of Apollonios of Tyana is, for the most part, a wild romance, containing a very small portion of fact to a great deal of uncontrolled fancy. It is fairly good evidence for popular and semi-popular beliefs of the third century A.D., but a very poor witness to historical or geographical fact. The passage Mr. Migeod quotes (III, 20) begins as follows: "There was a time when the Ethiopians, who are an Indian race, " lived here [i.e., in India], and there was as yet no such country as Ethiopia, but " Egypt had its boundary beyond Meroe and the Cataracts, thus containing the " springs of the Nile in itself and ending with the mouth of the Nile." The speaker goes on to tell of the Ethiopian king, one Ganges, son of the river of that name, and of superhuman size, beauty and wisdom: how he was murdered, and how the people were compelled by his ghost, coupled with plagues of all sorts, to execute his murderers; and finally, how he became reincarnated in the speaker, a Brahmin sage. Nothing is said of any migration into Egypt, though it is certainly implied that such took place some time. The war to which Mr. Migeod refers was one fought between Ganges and "the Scythians who dwell beyond the Caucasus." Anyone who will read but a few pages of Philostratos's curious work will see for

himself how little dependence can be placed upon it, and how likely such stories as this are to be the author's own invention, or borrowed from someone as unreliable as himself, rather than containing any fragment of history or of genuine popular tradition.

Yours faithfully,

H. J. ROSE.

ANTHROPOLOGICAL NOTES.

Royal Anthropological Institute—Removal to New Premises. The Council has purchased the lease of a house, No. 52, Upper Bedford Place, to which the offices of the Institute will be moved on 31st January next, on the termination of its tenancy of the present premises. In addition to the purchase price of the lease, considerable expense will be incurred in strengthening the floors to take the weight of the Library and to provide the stability requisite for a Lecture Hall. The Council was encouraged to take this step by a donation of £1,000 from a source which is at present anonymous. This has been supplemented by several donations of £100 each, but a sum of approximately £1,200 was still required when the Council issued an appeal to the Fellows for subscriptions to the Housing Fund at the beginning of November. At date a little over £500 has been received. It is hoped that Fellows who are in a position to assist will do their utmost and at an early date. The Council hopes that in more commodious and convenient premises it may be possible for the work of the Institute to be carried on with greater efficiency and that its activities may be extended. Whether this will be the case rests with the Fellows. It will only be possible if the Institute is able to enter on the occupation of its new premises free from debt. The amount in hand at the moment is barely sufficient to complete the purchase. 8

The Indian Section of the Royal Anthropological Institute. The Council of the Institute has authorised the institution of an Indian Section for the study of Indian archaeology and ethnology. The aim of the Section will be, in the first instance, to provide facilities for workers interested in Indian studies to meet both informally and at full sessions of the Institute. In addition it will co-operate with workers in India and aim at providing a centre for the co-ordination of the anthropological work which is being carried on in the different parts of that country.

In order to carry on work of this kind with any prospect of success, it appeared desirable that the Section should have a periodical publication for the furtherance of its objects. Having in view the undesirability of adding to the number of periodical publications already in existence, the Council decided to take advantage of the fact that Sir Richard Temple, owner and joint editor of the *Indian Antiquary*, was desirous of placing that publication on a permanent basis, and as a preliminary step had handed it over to a small private company. The Council, therefore, offered to take over the *Indian Antiquary* from the company, and this offer has been accepted. The Institute will, therefore, be responsible for the publication of the *Indian Antiquary* as the official organ of the Indian Section from 1st January. The *Indian Antiquary* will be obtainable by Fellows on payment of an annual subscription as in the case of MAN. Should it be found that any considerable number of Fellows is prepared to subscribe, it is hoped that it may be possible later to supply it to them at a preferential rate. Particulars of rates of subscription, etc. may be obtained from the Assistant Secretary of the Institute.

Rivers Memorial Medal. The first award of the Rivers Memorial Medal for anthropological work in the field has been made by the Council to Dr. A. C. Haddon, F.R.S., for work in New Guinea, Torres Straits and Borneo. The presentation will take place at the Anniversary Meeting on 27th January.

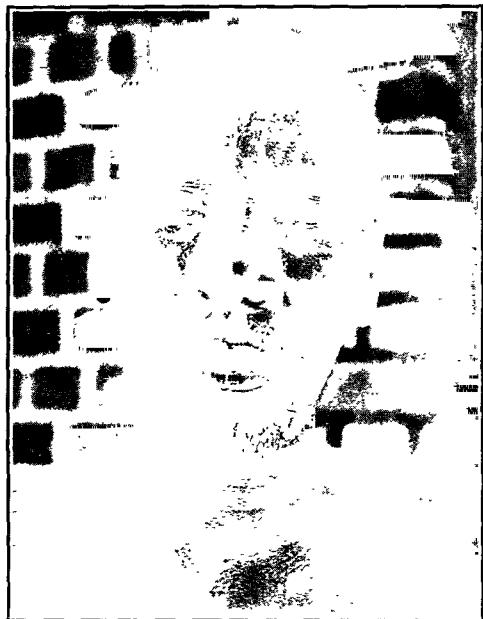


FIG. 1.

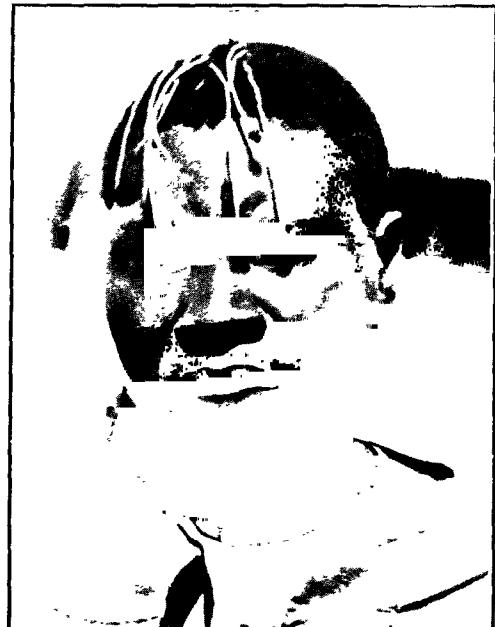


FIG. 2.

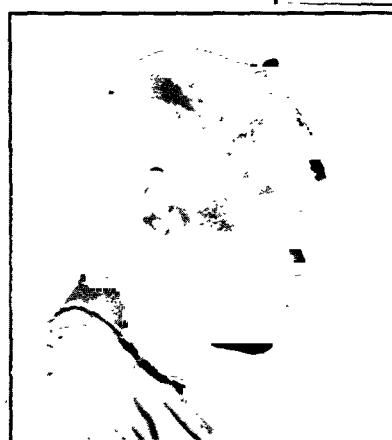


FIG. 4.

FIG. 5



FIG. 3.



MONGOLISM IN NYASALAND.

ORIGINAL ARTICLES.

With Plate A.

Africa, Central: Mongolism. Stannus.

A Note on Mongolism in Nyasaland. *By Hugh S. Stannus, M.D.* 9

In MAN. 1924, 130, Professor Seligman refers to pseudo-Mongolian types among Madi and Nuba natives of Africa and suggests that similar types found elsewhere in Africa should be recorded.

My impression is that such types are to be found among all tribes in the East African colonies. Nearly twenty years ago I made notes of these features and in an article on "Congenital Anomalies" * stated in reference to natives of Nyasaland: "a slight degree of epicanthus may be fairly often observed; more marked, it is seen sometimes associated with obliquity of the palpebral fissures, giving a regular Mongolian character to the face."

In a series of observations among the several tribes in Nyasaland some degree of epicanthus was noted, in proportions given below:—

No.	Tribe.	Epicanthus.			
		Slight.	Marked.		
27	Nyanja	-	-	3	0
110	Yao	-	-	10	6
23	Ngoni	-	-	2	1
61	Tonga	-	-	4	2
50	Wemba	-	-	12	0†

The photographs show respectively (Fig. 1) an old Yao man and (Fig. 2) a young adult Ngoni man, both exhibiting high cheek bones, with characteristic face shape, narrow oblique palpebral fissures, broad flat bridge to nose, but no epicanthus. (Fig. 3) shows a somewhat similar Mongolian type in profile. The lower figures are both normals; Fig. 4, a Ngoni; Fig. 5, a Yao, for comparison.

HUGH S. STANNUS.

China: Archæology. Buxton.

Early Man in China.‡ *By L. H. Dudley Buxton, M.A., F.S.A.* 10

When Dr. Andersson was good enough to show me the result of his excavations three years ago I was not a little astounded, especially since I had recently been looking at the material which had been collected by the Japanese who have found in their islands an extensive series of sites which can be vaguely described as "Neolithic" and which were inhabited by people who were akin to the modern Ainu. On the mainland, in Manchuria, the Torii found a culture which is also of the same crude type, though we do not know the people with whom it was associated. Unfortunately Torii's§ publication leaves much to be desired; it is difficult to be sure from his illustrations of the exact type of his pottery. But Andersson, working both in Honan and in Manchuria, not very far from where the Torii worked, has found a culture containing very different elements. The people appear to be physically not unlike the present inhabitants of northern

* "Congenital Anomalies in a Native African Race." *Biometrika*, Vol. X, No. 1, Apr., 1914.

† "Some Anthropometrical Observations among the Natives of Nyasaland." Manuscript deposited in the Library, Roy. Anthropol. Inst.

‡ "The Cave-Deposit at Sha Kuo T'un in Fengtien." J. G. Andersson. Plates xii, 26 text figures. "Palæontologica Sinica," Series D., Vol. I., Fas. 1. "An Early Chinese Culture." J. G. Andersson. Plates XVII, 15 text figures. *Geological Survey of China Bulletin* No. 5, 1923.

§ *Tokyo Imperial University Sci. Journ.*, Vol. XXXVI. 4 1914; 8, 1915.

China, but, until Dr. Black's monograph on the subject appears, little can be said on that point.

Two sites have so far been explored and the results published. One is a cave at Sha Kuo T'un in Fengtien, the other a very extensive village site at Yang Shao in Honan (see Fig. 1). This last site is over 600 metres long, 480 metres broad with an average thickness of 3 metres, the whole of this area being full of pottery and other human refuse. A further characteristic of the site is the occurrence of pockets from two to four metres in diameter and from a half to two metres deep. They contained very broken pottery and Andersson believes that they were not huts because of the bulky pots which the people possessed presupposing, he thinks, larger dwellings. I do not feel convinced that his arguments are cogent. In any case, the site is certainly that of a village. The cave contained a large number of human remains and the excavators consider that it was visited for ritual purposes,

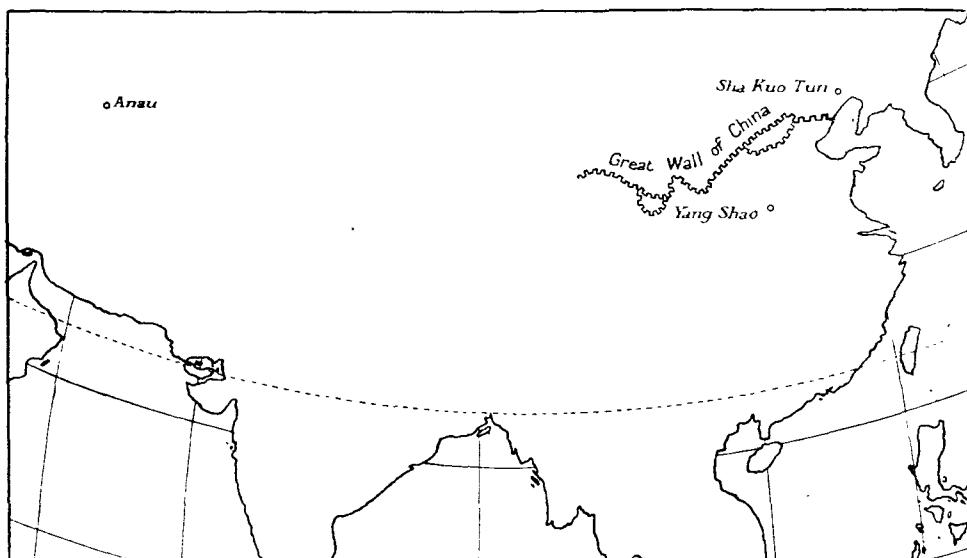


FIG. 1.—SKETCH MAP TO SHOW THE POSITION OF THE SITES EXCAVATED BY DR. ANDERSSON.

possibly human sacrifice. Here, again, I hardly feel convinced that we have not an ordinary rock-shelter, which was used at times for burial.

The artifacts found on the Honan site are very numerous, and include celts, spinning whorls, rectangular knives like those used to-day in the Kaoliang districts, and rings; arrowheads of slate are common, but bone and even mussel shell is used as material for these implements; needles and awls are also found. The most numerous remains are, however, potsherds.

In Fengtien the fragments were very small; in Honan more complete vessels were found, some of which could be restored or reconstructed into their original shape. Andersson's classification is far from clear, and apparently in each brochure he adopts a different grouping. The difference between the two sites is small, and we appear to be right in considering, with Andersson, that both are only local varieties of the same culture. One of the most important deficiencies in his monograph is the mention and figuring of pottery from other sites, the names of which, except in one case, he omits to specify. It is, therefore, impossible to form any idea of the distribution of the culture in China.

The description which follows is largely based on Andersson, but I have enlarged his summary where this is not clear. Broadly speaking, we may accept

two classes of ware, coarse and fine. The coarse is evidently of a domestic character, and cooking pots of this type survive in China to-day. It resembles the Torii's Manchurian Culture, and is extremely like the "Neolithic" culture of Early Japan.* This seems to be essentially an Oriental culture, and is widely spread over the Far East. The ware is coarse, sometimes as much as a centimetre in thickness, and the clay is mixed with abundant quartz grains. The colour is usually greyish-brown or brick red, though light grey sherds are very numerous in Honan; they are absent from Fengtien. The pottery is usually handmade, though Andersson believes that a wheel was occasionally used. It is usually decorated with a textile impression, due probably to the mat on which it was built up. Sometimes string marks appear. I am inclined to think that these are due to a desire to reproduce the old pattern after the technique had improved and the textile support was no longer used. Incised patterns also occur.

In addition to the technique of this pottery, there are indications of form, to which Andersson draws attention, which serve to link this coarse ware with modern Chinese culture. I reproduce his suggested parallel between the modern Li character and some earlier forms. The archaic form of the character which means an earthenware pot is a pictogram representing a clay pot used in the Chou dynasty, and closely resembles bronze tripods of that dynasty which survive. In Yang Shao pots were found just under nine inches high, made of coarse ware with wide hollow legs, the interior of which form an integral part of the interior of the vessels. Except that the legs are drawn too widely splayed, and the neck is much exaggerated, the early form of the character is a very good diagram of this class of pot as found by Andersson (Fig. 2, a). The latter development of the character can be followed from the figure. It would seem as if this remarkable tripod, a curious tumbler-form, and the cooking pots are modern survivals in China of this old culture. Some of the other survivals noted by Andersson, especially the rectangular knife, are of great interest; others seem far-fetched. Space does not allow their discussion here.

The second type of pottery consists of fine ware made of carefully-worked clay, not intermixed with mineral particles. It is usually light brick-red in colour, with a smooth polished surface, and can be divided into two classes—monochrome and painted. The vessels seem to have been of moderate size and simple shape, mostly bowls. They are usually well-burnt, and, with a few doubtful exceptions, are wheel-made. The surface of the monochrome vessels is often burnished and blackened, and some of the shapes are like tumblers and resemble those of later Chinese vessels. Possibly this class also may be considered as being essentially Oriental in origin, and the grouping of all the fine ware together, as is done by Andersson in the Honan paper, does not seem to be justified. He has modified the classification in the Fengtien paper.

The third and most interesting group for our purpose is the fine polychrome ware. The vessels are mostly small hemispherical bowls, possibly with a flattened bottom, and decorated with black painting. White painted vessels are found.

* An account of this will be found in the archaeological monographs published by the University of Kyoto (*Archaeological Rep.*, 1920, 1921), with excellent illustrations. A summary of the recent work in Japan will be found in H. Matsumoto, "Notes on the Stone Age People of Japan." *Am. Anthropol.*, N.S., XXIII, 1921, pp. 50-76.

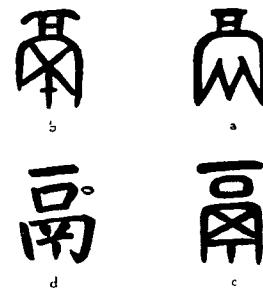


FIG. 2.

(TEXT FIGURE, ANDERSSON, "AN EARLY CHINESE CULTURE.") A, B, C, EARLY FORMS OF THE CHARACTER LI, FROM THE SHUO WEN KU CHOU PU. A IS A GOOD DIAGRAMMATIC REPRESENTATION OF THE LI FOUND BY ANDERSSON. D REPRESENTS THE MODERN FORM OF THE CHARACTER.

though rarely in Honan. I cannot find a record of such in the Fengtien cave. I have here reproduced parts of plates IX, fig. 3, and XIV, fig. 4. The original papers are well illustrated, and there is one plate in colour which, though not quite accurate, gives an excellent idea of the pottery. Those here reproduced will give a good idea of Andersson's careful work in black and white. His map is less satisfactory, and I have had it redrawn.

Those with a plain rim are decorated with a black band round the rim or a row of triangles between two horizontal lines. Sometimes, as in Fig. 3, a trellis pattern is used. In other cases the pattern is more elaborate. In the vessels with flaring rims, *e.g.*, Fig. 3, 4, the decoration may be simple and sometimes be made up of curved bands, triangles, dots, and so on. The general form of the patterns can be seen from the figures: the black indicates black, the dotted surface red.

The most important local difference is the occurrence in the cave of bowls and decorative patterns which are not found in Honan; otherwise the cultures are very

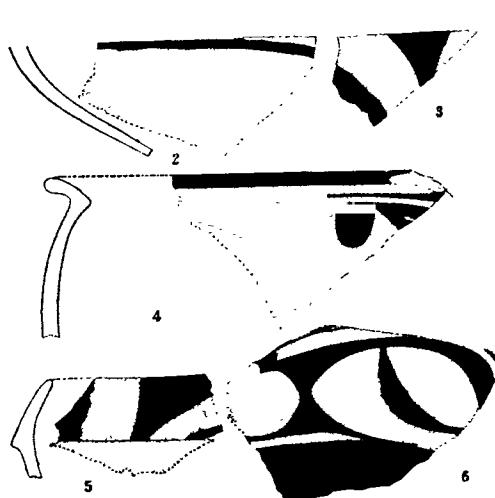


FIG. 3.—(PART OF ANDERSSON, "EARLY CHINESE CULTURE," PLATE IX). POTSHERDS OF PAINTED TYPE FROM YANG SHAO TSUN IN HONAN. black=black, stipple=red.

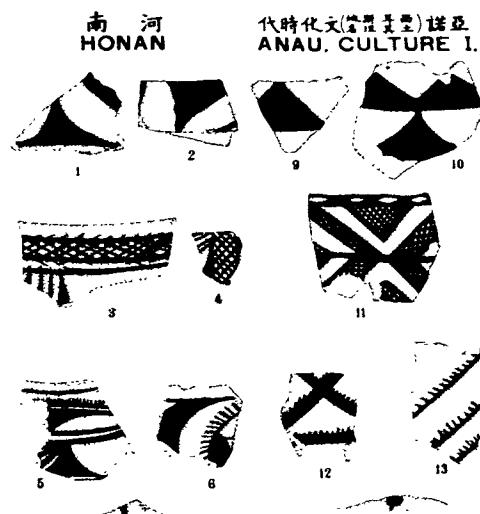


FIG. 4.—(PART OF ANDERSSON, "AN EARLY CHINESE CULTURE," PLATE XIII). COMPARISON OF PAINTED POTTERY, HONAN AND ANAU. 1, 2, 4 ARE FROM YANG SHAO; 3, 5, 6, ARE FROM UNSPECIFIED SITES.

similar, and stress must be laid on the fact that no metal objects were found on either site.

Andersson has drawn attention to the resemblance between his wares and those from Anau. The general distribution of this type of fine polychrome pottery is known to be wide: it appears in Asia Minor and Babylonia, and extends to the east of Persia. Sir Aurel Stein informs me that a similar ware has been observed by him in Seistan. It extends to Anau, to Tripolje, and is not unlike the ware found in Thessaly, and even further to the West. I have seen some fragments found in Hungary which, although they seemed to belong to the same family, presented certain differences.

It would appear, however, that we have in this somewhat remarkable pottery a type of ware which is known to extend, since Andersson's work, from the Mediterranean to the Pacific. I have shown in the outline sketch map, Fig. 1, the sites in China; the cave site Sha Kuo T'un is the most easterly and the Honan village, Yang Shao, the central site. I have also included the position of Anau. The geographical details would only serve to confuse the map, but the position

of the Wall serves roughly to show the dividing line between the plain of China and the upland plateau of Mongolia. The entrance to the plain from the west is comparatively limited, although it is possible that the deserts may not have been so impassable at other times as they are at present. Further details of this distribution are at present lacking, although it is known to extend into Kansu. It is to be hoped that Andersson will see fit to publish further details of what he describes as "new localities," including Pu chao, from which some specimens are published. Is this the Pu chao in Shansi?

The date of the culture is of great interest. It would seem as if one period is suggested for the whole culture. If this is so, then it must have been of some duration, at least in Honan, to account for so great an accumulation of *débris*. On the other hand, I do not feel entirely convinced that at present this can be said to be proved. A further investigation of the sites may show differences. At present the discoveries are mostly limited to masses of broken material. The sites may, therefore, to a certain extent, at least be *remaniés*, and until a site which contains objects in better condition is forthcoming, it would seem hardly possible to pronounce definitely on this point. It is clear, however, that all the material on the whole belongs to the same general period. At present the dating is uncertain. It is hardly possible to date a site by the type of pottery at one end of Asia by supposing that a similar type at the other must have a similar date, and any calculations on the basis of the migration of culture must necessarily be in the nature of pure conjecture. Two means remain. Either historical or palaeontological. In regard to the latter, we find that the commonest animal in Honan is the pig, apparently of the same species as the domestic pig of China to-day, a most interesting parallel for the continuity of culture, but of no chronological value. Andersson, however, points out that the land surface in Honan, and probably over a wide area, has changed since the site was inhabited; indeed a ravine 40 feet deep has been cut through the site and the drainage has been considerably altered. We know, however, that in some parts of the Chinese plain considerable changes of surface have taken place since historic times. This, then, provides no chronological data.

The historical method remains. Unfortunately we have few indications here. Probably the culture is slightly anterior to the early Chinese dynasties; bronze seems, as Andersson points out, to have been largely used in the Hsia dynasty, which is usually dated about 2000 B.C., but it is dangerous to argue about dates over so wide an area.

On general grounds, I should be inclined to suggest that the Fengtien cave may be not later than 1500 B.C., and is possibly earlier, and that the Honan village is the earlier site. This would allow sufficient time for the spread of the culture from the west, and at the same time would allow time for the earlier Chinese dynasties to develop their culture. The absolute connection of the cultures must depend on the synchronisation of dates, and at present we have not sufficient data to attempt to do this.

L. H. DUDLEY BUXTON.

Britain: Archæology.

Moir.

Solutrean Flint Implements in England. By J. Reid Moir.

11

The question as to whether flint implements, and other human relics, of Upper Palæolith age, have been found in England, buried beneath various deposits in the open country, is of much interest, and importance, to students of prehistoric archæology. The inherent difficulties of this problem have, unfortunately, been accentuated by the frequent use, by archæologists, of the misleading term "Cave Period" to describe the Upper Palæolithic epochs—a phrase which, in itself ridiculous, has apparently led some investigators to entertain the preposterous

notion that the people who lived in these epochs resided solely in caverns, and "refused" to exist elsewhere. My friend, Mr. Miles Burkitt, has recently published (MAN, 1925, 3) another short, and interesting, account of various items, found in England and on the Continent of Europe, which he refers to Upper Palæolithic times, and has, I notice, made himself responsible for the following statement: "The problem of how far Solutrean implements occur in England has long exercised prehistorians. The laurel leaf is by no means a typical tool, unless it is found stratigraphically in Upper Palæolithic deposits, or associated with quaternary fauna. Single tools, especially when they are not massive, easily get displaced, sometimes to considerable depths, and a laurel leaf found isolated may be of almost any date from Solutrean to Bronze Age times."

I have carefully studied this pronouncement, and conclude that its meaning is as follows:—

- (a) That an implement of laurel leaf form is not to be regarded as, by any means, typical of the Solutrean period—unless
- (b) such a specimen is found in its proper stratigraphical sequence in an Upper Palæolithic deposit of Solutrean age, and accompanied by a fauna such as is known existed in Solutrean times;
- (c) that single flint implements, unless they are of large size, are apt to be translated, by some unspecified agency, to considerable depths below the horizon to which they in reality belong; and
- (d) that, therefore, isolated examples of laurel leaf implements may be referable to almost any age from Solutrean to Bronze Age times, *i.e.*, to the Magdalenian, Azilian, Neolithic, and Bronze periods.

Now, this statement of Mr. Burkitt's is, to me, in the highest degree disturbing, as if a "landmark" in prehistoric archæology like the Solutrean blade is to be cast aside as of no typological value, then the very foundations of the edifice we are all helping to build are indeed in a parlous, and unsafe, condition. I, therefore, made haste to refer to the works of some well-known authorities in order to see whether they share my friend's disquieting views on the Solutrean laurel leaf implement—and to my great relief I found that they do not.

I would quote:—

- (a) Professor Henri Breuil ("Les Subdivisions du Paléolithique supérieur et leur Signification," p. 190):—
"La technique solutréene se perfectionnant la feuille de laurier, caractéristique du plein Solutréen."
- (b) Professor Hugo Obermaier ("Fossil Man in Spain." English translation, p. 104, and Fig. 43):—
"The climax of this industrial stage is found in the Early Solutrean, distinguished by the 'laurel-leaf point,' carefully worked on both sides."
- (c) Professor Marcellin Boule ("Fossil Men." English translation, pp. 249–250, and Fig. 157):—
"The Solutrean, which succeeds the Aurignacian in some localities, is marked by extremely able and careful work in stone. The characteristic types are narrow points known as 'willow leaves,' larger points called 'laurel leaves,' and shouldered points ('pointes à cran')."

It is quite evident that these authorities regard the laurel leaf implement itself as typical of the Solutrean period, and as of equal value in this respect with the shouldered point, upon which Mr. Burkitt in his note lays such particular stress. I would wish now to ask him whether he considers the famous Volgu blades, which were not found in their proper stratigraphical sequence in Upper Palæolithic deposits, nor associated with fauna of any sort or kind ("On n'a d'ailleurs rencontré dans les mêmes fouilles aucune trace de métaux, ni d'os d'animaux." "Les Silex

de Volgu," p. 2) are by no means typical tools of the Solutrean period, and that they may be of almost any date from Solutrean to Bronze Age times?

This matter is of especial interest to me as I was privileged, recently, to be able to publish an account (*Proc. P.S.E.A.*, Vol. IV, Part 1) of a series of laurel-leaf blades, of great beauty, found in various parts of Suffolk and Cambridgeshire. Three of these blades were found at a depth of, at least, 12 feet (alluvium 4 feet and gravel 8 feet) at a site near Constantine Road, Ipswich. The strata at this spot are known, intimately, to me, and it is not a reasonable supposition that the specimens, which were all found at the same level, slipped down, through cracks, from the top to the bottom of the gravel, nor that they were borne to the place where they were found by that hard-worked agent of archæological disturbance, the burrowing animal. Another of the blades described by me was found under 9 feet of gravel at Bury St. Edmunds and in this case also there can be no reasonable doubt that the specimen belongs to the horizon at which it was found. My late lamented friend, Mr. E. T. Lingwood, who drew the specimens for my paper, told me, a few months before his death, that he felt he had represented the flake-scars upon the blades as of a more concave form than is the case upon the implements themselves. I think this is, perhaps, true, as the flaking of these specimens is typically Solutrean, though not, necessarily, in my judgment, produced by pressure, as seems to be, for some unstated reason, so widely thought to have been applied in the manufacture of Solutrean blades. I do not know what Mr. Burkitt's opinion is regarding these East Anglian specimens, but I have a suspicion that for him to accept them as Solutrean would constitute a somewhat severe shock to his views, as I notice he states that "it would be unwise to presume . . . that the Solutrean "race really inhabited Great Britain." I trust, however, that he will not attempt to relegate the gravel, under which these Suffolk blades were found, to Neolithic times, as that would be not only a baseless assumption, but one calculated to make confusion worse confounded.

It is clear that we are invited to look upon an isolated, and diminutive, flake found at Lakenheath, and showing some resemblance to a Solutrean shouldered point, as evidence of what is known as Solutrean "influence" in England. Regarding this specimen I would like to ask the following questions:—

- (a) Was the flake discovered in "isolation"?
- (b) Was it "found stratigraphically in Upper Palæolithic deposits," and "associated with quaternary fauna"?

If the reply to these queries is in the negative, then, in view of the fact that, as the "shouldered point" implement is not considered (*see* authorities quoted above) as being any more typical of the Solutrean period, than the "laurel leaf" blade, why should this particular specimen be elevated to a position of importance, and not regarded as being, possibly, of "almost any date from Solutrean to Bronze Age times"?

For some time past I have noticed a peculiar inclination on the part of some investigators to put forward certain extraordinary arguments against the view that flint implements found under superficial deposits in East Anglia, and elsewhere, are of Upper Palæolithic age. But I am unable to imagine any reason for believing that England was, as it were, "out of bounds" in Upper Palæolithic times, and it is my hope that this note may initiate a discussion upon this important matter in the pages of *MAN*. J. REID MOIR.

Ethnography: Siam.

Schebesta.

The Sēmangs of Patalung. *By the Rev. P. Schebesta, S.V.D.*

My object in visiting Siam was to collect information about the Sēmangs of Siam and, if possible, to determine whether the Sakais extend into that region. 12

To dispose of the latter point first, there are no traces of Sakais in Siam; so far as I am aware at present, the Sakais do not extend to the Northern bank of the Perak river. The Sēmangs, on the other hand, extend far to the Northward; but I am not in a position as yet to indicate their actual limit. I could not get reliable information about it anywhere. But they are said to be most strongly represented in the province of Puket and the Patalung-Trang region. I am also unable to give a definite answer to the question whether the Sēmangs, or tribes resembling them, are to be found even further to the Northward. Still, it would be advisable to bear in mind the existence of the Porr tribes, which on Siamese territory are known as Chong. According to reports I have received and pictures that I have seen, it is probable that they are Negritos, who are however much crossed with other races. They inhabit the Cardamon Mountains, and are also known by the names of Tamret and Samret.

The Sēmangs on Siamese territory are estimated by a Phya, who gave me some information about them, at approximately 3,000 in number; but that is assuredly incorrect. They are certainly less numerous in Siam than in Malaya. The Siamese call them Ngo, which means crisp-haired and is also the name of a fruit (the *rambutan*) which has the appearance of a frizzy-haired head. The very name seems to indicate that the Siamese in that region are only acquainted with crisp-haired junglefolk.

On my return journey I made an attempt to get into touch with the Sēmangs of Patalung. As my time was limited, I am unfortunately unable to report more than a partial success of the attempt. Nowadays Patalung and Trang are linked together by a fine road which crosses the mountain range that runs down the Peninsula. That is the home of the Patalung Sēmangs, a tribe of some 30 to 40 persons. The peculiarity of this tribe is that their *kēpala* or head is a woman, named Isan; I have nowhere heard of a similar state of things.

I did not see the quarters of this tribe, but I met six members of it who were on the way to Patalung to buy rice. There were two men, two women and two fairly big boys. During our interview only the two men and one of the women, who was very talkative, took part in the conversation, which I could only carry on through interpreters, as the Sēmangs could not speak Malay, though they spoke Siamese tolerably well. They did not seem as timid as the Sēmangs in Perak: though they were at first exceedingly shy, and the two boys made a move to depart when I appeared, they were very soon at their ease again.

All the six seemed to me to be extraordinarily small. This had not struck me in the case of the Perak Sēmangs, but that may have been due to the environment, as I always saw the Perak Sēmangs in the jungle, while these were in the open. The colour of their skin was very dark, resembling that of the Jahai. The nose was distinctly negroid, the eyes level, the expression of the face very childlike. The hair of all the individuals was crisp, and not shaved (as is the practice of the tribes of Ijok and Lenggong) but like the hair of the Jahai. I noticed no decoration of the body. One woman had her earlobes pierced. Their clothing was of cloth, that of the women being ampler than I have been used to observe among other tribes. No special skin ailments were noticeable, but both of the men had some scars of wounds.

One of the men was armed with a *sumpitan*, which I succeeded in buying from him. It differs from the Perak *sumpitan* in having a much smaller mouthpiece. The *sumpitan* had been made by the man who was carrying it. The quiver that goes with it is wide but very short, without any lid or any kind of decoration. It was full of small tubes containing the poisoned darts. They call the *sumpitan* *baláu*, the quiver *malú*, the darts *honlig'-sogen*. The men informed me that the bow is also used and is called *canú*. The bowstring is called *aeui* and the arrow *bilá*.

I was unable to ascertain the name of the tribe, but succeeded in collecting a short list of words, which indicate a somewhat close connection with the Jahai. However, some strange words also occur. They count up to two : 1 = *nai*, 2 = *komam*; "three" is rendered by *mapu*, "many," and alternatively by *tiga*. This last is of course very interesting as it affords evidence that these tribes had had relations with Malays or with other Sémangs who are in touch with Malays. One of the men also knew a few Malay phrases.

As these people were anxious to leave, I had to let them go. I made an arrangement with them to accompany me on the following morning to their quarters, which they readily promised to do. But I had my doubts about it, and unfortunately the event justified them. During the night the tribe departed, being impelled, so I was informed, by fear; and I did not succeed in finding their quarters, though I searched for them next day along the way towards Trang. The Siamese natives, who are probably even less interested in such matters than the Malays, all said they did not know where the Sémang camps were, as they were constantly shifting them.

The Patalung Sémangs were visited in 1907 by the then king of Siam, who made a brief description of them, of which the following is an abstract.

They are of medium stature, and have broad noses: their lips are not very thick. Their hair is crisped. The men often leave a fringe of hair standing round the head. The women let their hair grow long, others shave it. The colour of the hair is not as dark as with the Siamese. The men are powerfully built.

Their huts are said to resemble tents. In the rainy season they take refuge in caves. Their camp, which is pitched near some stream, has a free space left in the middle of it.

Their food consists of roots, but also of rice, which they buy elsewhere, and fruits, as well as meat, particularly the flesh of monkeys.

They have many children: the women are said to bear a child every year.

They are afraid of the rainy season, and prefer the hot season. When they have fever, they smear their faces with white paint or hang a cord round their necks.

Unmarried girls stick beads and flowers into their earlobes, which are pierced. Women wear bamboo combs and bracelets. At a wedding the bridegroom gives the father and mother of the bride a cloth apiece. Red ones are particularly prized.

As weapons they use the *sumpitan*, which is well made and carefully kept. If anyone is guilty of an offence, the headman takes his *sumpitan* away from him, and they fear this punishment very much. The *sumpitan* is called *bolau*, the poisoned darts *bilá*. They also use spears.

They revere a deity who requites good and evil. Trees are inhabited by spirits, which are female and must be revered, otherwise they will kill one. These demons have sickle-shaped weapons, with which they cut the abdomen open and proceed to devour the entrails of their victim.

There are four sorts of spirits: (1) *Ya*, that is to say the human soul when it is separated from the body. After its departure from the body, it waits till it can be born again. It waits for a new body. If there happens to be one in the womb it enters into it. If within six months' time it does not succeed in finding a body, it becomes a ghost which worries people. (2) *Rob*. This is the human soul which leaves the body during sleep and walks about. It is thus that dreams occur. (3) *Semanat*. A certain spirit, only mentioned in connection with some particular individuals, which is sent to worry other people. (4) *Badi*. This demon originates from animals. If it takes possession of men, they become like animals. They are kept at a distance by medicine known to the medicine man.*

* The names numbered 3 and 4 are from Malay *sémangat* and *badi*, though the descriptions do not quite coincide with the Malay meanings.

Corpses are not buried deep. The place is then abandoned on account of fear of spirits and fear of tigers, who are said to eat the corpse. Malays seek for the bones of the Sēmang in order to grind them up and smear the resulting powder over their faces as a medicine.

Such are the main points of King Chulalongkorn's account. His informant was assuredly the Sēmang boy whom the king took to his Court and had educated there. His name was Kanam.

P. SCHEBESTA.

REVIEWS.

Britain : Archæology.

Haverfield : Home.

The Roman Occupation of Britain : being six Ford Lectures delivered by F. Haverfield, now revised by George Macdonald with a notice of Haverfield's Life and a list of his writings. Oxford. The Clarendon Press, 1924. 18s. **13**

Roman York. The Legionary Headquarters and Colonia of Eboracum. By Gordon Home. London. Benn. 1924. 12s. 6d.

The six Ford lectures on the Roman occupation of Britain, delivered by the late Professor Haverfield in 1907, were, after his untimely death, found to be in part prepared for publication in an extended form. The "recasting of the whole " six on the lines which he had marked out " has been admirably carried out by Dr. Macdonald. The first lecture presented a most interesting summary of the work of pioneers in Roman studies in Britain, such as Stukeley, Gale, Horsley, and William Camden, the last named in some respects the greatest of all. In the dedication it is explained that the publication of the book is a tribute from the University of Oxford to the memory of Camden, and to the latest holder of the Camden Professorship, the author himself.

No better introduction to the problems of Roman Britain could be offered to a student than this. Haverfield's views on the Roman conquest in its geographical aspect are set out in detail in Lecture II; this lucid summary illuminates the history of the occupation. That occupation, on its military side, is discussed in Lecture III; and in two further lectures the civilisation which developed in the rear of the protective screen thus provided is described.

The book is well produced and illustrated. Noteworthy as representative, one of the military occupation, the other of the civilised life of the province, are the fine photographs of Hadrian's wall at Hotbank and the baths and basilica at Wroxeter.

The fascinating, but difficult, problem of the history of Britain in the fifth century is dealt with in the last lecture. The probable course of events is illustrated by interesting parallels—the evacuation of the Danubian province of Noricum Ripense in the fifth century—and the conquest of Mecklenburg by the Germans in 1170-1280. Complete as the destruction of Roman civilisation seems to have been, is it, however, not misleading to say that "racially, topographically, "culturally, ancient Rome has nothing to do with modern Britain"? The influence of the Roman road system in determining, in the Saxon and mediæval periods, the sites of towns and of monasteries was very marked, and these roads were surely the channels along which trade and civilisation flowed back into England.

Haverfield, in an interesting passage of the book under review, deplores the incompetence of writers on Roman Britain in the nineteenth century, and incidentally describes the existing studies of Roman Lincoln and Roman York as well-nigh valueless. Of the improvement in scientific outlook and competence, which he himself did so much to foster, Mr. Gordon Home's book on Roman York

is a good example. It is a careful and detailed study, presenting all the facts known about the city in Roman times, and describing the existing remains and the chance finds which have been recorded or preserved. Eboracum was a legionary camp, not a centre of civil administration, and the book illustrates the distinction, which Haverfield emphasises, between the two Britains. Whether the site was occupied by the Brigantes is unknown; no remains of the Early Iron Age have been found in it; but Mr. Home's view that it was the Brigantian capital is, in view of its geographical importance, not impossible. The foundation of the Roman city dates from about A.D. 71, when the war with the Brigantes was being pressed on with vigour by Petilius Cerealis. Till 119-120 it was the headquarters of the IX Legion. After the destruction of that legion its place was taken by the XI Legion, which remained at Eboracum until the close of the Roman occupation. York was always, on account of its position, the most important legionary camp in Britain, and was closely connected with the campaigns of several Emperors.

With such a history it is remarkable that so little is known topographically of Roman York. Finds of which the exact site are recorded are lamentably few; and of the plan of the castra little more than its boundaries—the line of the walls—is known. The record of provenance of the majority of the objects in York Museum, as Mr. Home notes, is inadequate. A similar story of wasted opportunities can be told of other Roman sites occupied by modern towns; fortunately a new spirit is abroad, and the importance of accurate topographical record is now fully realised. Mr. Home's book will encourage the citizens of York to assist Dr. Collinge, the present keeper of the Museum, in recovering the history of their city.

Mr. Home's greatest problem has been how to present a continuous history of Eboracum with inadequate material. He has done it by sketching the general history of Britain, noting, where local records fail him, the probable part taken by the fortress in a given episode. It is, perhaps, the only method, but it has its dangers. What is the value, in a serious historical study, of such a conjecture as "there is certainly a possibility that Eboracum was the scene of the murder [of Carausius]"? Nothing beyond the fact of the murder is known.

The book is well got-up and illustrated, but the absence of numbers to the plates makes reference difficult, and there are no indications of scale, which is a serious omission. A few slips, such as "Mother Gods" (plate opposite p. 146), and printers' errors such as castramentation (p. 126), have been noticed. C. F.

Ethnology.

Perry.

The Origin of Magic and Religion. By W. J. Perry, M.A. London: Methuen, 1923. Pp. 212. Price 6s. **14**

The Growth of Civilisation. By W. J. Perry. London: Methuen, 1924. Pp. 224. Price 6s.

Everyone will welcome the restatement in a popular form of the startling results obtained by the same author in "The Children of the Sun" and several technical works. But the books under review do much more than recapitulate specialised investigations. They cover an enormous field of research and present a wide and comprehensive view of the whole evolution of spiritual and material civilisation. A mass of scattered phenomena are knit together into an engagingly simple unity and are related, as they should be in any genuine archaeology and anthropology, to the vital problems of sociology. The result is a system which is not only calculated to appeal to the layman, but must be stimulating and suggestive to the specialist.

It is indeed true that synthesis would be more convincing if the facts on which Mr. Perry relies had been more carefully verified, if the unifying principles had been

applied in a more scientific manner, and if the omissions had been less one-sided. For instance, it is disconcerting to find a vital argument in both books based on the assertions that the beehive tomb appears in Crete about Middle Minoan I, *circa* 2100 B.C., was modelled on pyramids of the XIIth dynasty, and was due to Children of the Sun who came into power with the Vth dynasty: for, apart from the apparent contradiction between the statements themselves, the tombs in question in Crete began in Early Minoan I and so have nothing to do with Vth dynasty Children of the Sun. Incidentally, the phrasing of the subsequent sentences would lead the layman to the erroneous conclusion that corbelled tombs in the British Isles covered a wealth of gold ornaments.

In the second place the use made of the principle that the same phenomena must all be referred to a single origin—a useful heuristic device—might lead to confusion. To Mr. Perry, all polished stone implements, all copper daggers, all clay figurines are the same, and we are allowed to infer that the incised spirals on Danubian vases are very like the painted rectilinear ornaments of Susian pottery. Such sameness exists only in the night, where all cows are black: prehistory as a science is based precisely upon the recognition of significant differences in such artifacts. In any case, the reiterated claim that “wherever the polished stone celt is found, “it is certain that the archaic civilisation has exerted its influence,” is in flat contradiction with other less questionable statements that such implements characterise the earliest food-producing cultures, e.g., in Sumer and the Danube valley, which antedated the archaic civilisation and were never affected thereby.

Nor is it quite clear that Mr. Perry’s appeals to out of the way corners of the earth produce really cogent arguments in archaeology. Mr. Perry prefers to invoke the rather fragmentary and isolated monuments of the Pacific and Central America, rather than the European remains which have been much more thoroughly studied and systematically interpreted. No objection indeed could be taken to the use of the former if, when venturing into the better known provinces, Mr. Perry had displayed a fuller consciousness of the difficulties. But so many inconvenient but notorious facts have been ignored. In the chapter on European megaliths we do not hear a word about the typology established beyond cavil for Scandinavia by Montelius and Sophus Müller and successfully applied to Spain by Bosch Gimpera, Leeds, and Obermaier, nor about the teeth of our Long-barrow men. Though a whole chapter is devoted to “Givers of Life,” the form of amulets is not discussed. Yet beads are the most typical articles in the furniture of Atlantic megaliths, though the pregnant types, such as the *perles à aillettes* of South France, would be found to go back to specifically Aegean models foreign to Egypt. And is it fair to ignore the results of Abercrombie, Crawford, Déchelette and Piroutet, connecting the earliest traceable metallurgy of Britain and France with currents from Central Europe?

In comparative religion the same method is adopted. One would have imagined that the real development of human thought could be best traced in India where it is illustrated by the Vedas, Brahmanas, Sutras, and other contemporary documents, and where the foreign influences, such as Hellenism and Mohammedanism, are equally well known. But no: “the history of thought,” we are told, “is hard to understand in India . . . It is in the more outlying parts of the world where the movements of culture have been fewer, that the mode of derivation of ideas from the archaic civilisation may be best understood.” And so we are hurried to the Celebes, where the development of a couple of thousand years may be filled in by conjecture. The fragmentary traditions of Oceania do certainly fit very beautifully into Mr. Perry’s scheme and are themselves illuminated thereby. But proofs drawn from completely documented systems of thought would have a higher demonstrative value.

I have merely cited a few instances drawn from my own province of research to show the objections which make me hesitate to accept Mr. Perry's simple synthesis or the speculative conclusions based thereon. Still, his patient researches have revealed a number of similar phenomena, widely distributed throughout the world, that any archæologist and anthropologist in the future must take into account.

V. G. C.

Malaya: Ethnography.

Evans.

Studies in Religion, Folk-lore, and Custom in British North Borneo and the Malay Peninsula. By Ivor H. N. Evans, M.A. Cambridge: University Press, 1923. 20s. net. **15**

The first part of this book is concerned mainly with the customs, beliefs and folk-tales of the Dusuns of British North Borneo, and contains valuable material which will be of great help to anyone undertaking a comparative study of the folklore of Indonesia in general. The tales have many points of resemblance with those of other Indonesian tribes, and many of them are beast stories. This part of Mr. Evans's work may be regarded as a kind of amplified appendix to his book on North Borneo which was noticed in MAN some time ago.

The second part deals with analogous matters among the natives of the Malay Peninsula, especially the so-called aboriginal wild tribes, though a few Malay customs and tales are also reported. The most interesting portion is concerned with the beliefs, magical formulæ and folk-tales of the Negritos of the North of the Peninsula, regarding whom there is still much to be discovered. The formulæ are apparently in an archaic variety of their native language and are often decidedly obscure. It is interesting to notice that Mr. Evans's researches have done something to rehabilitate the work of Vaughan Stevens, although much of what he reported is still quite unconfirmed. Further investigation on the spot will, it is to be hoped, clear up many matters left doubtful. In the meantime the work under review is a valuable addition to our knowledge. Mr. Evans has been careful to note the particular regions where he collected his evidence, and his work bears all the marks of scientific accuracy.

C. O. BLAGDEN.

Africa, East: Ethnography.

Norden.

White and Black in East Africa. By Hermann Norden, F.R.G.S. Witherby. 15s. 1924. **16**

This book deals with a journey through Kenya Colony and Uganda, and although, for the most part, it covers ground which is now well known to many, it is written by an acute American observer, who has the natural gift of spotting many aspects of life which the resident rarely considers worthy of notice. The author, too, is blessed with a sense of humour which helps to render his story attractive.

We will not deal with his vignettes of the idiosyncrasies of white colonists and others, being more concerned with the peeps of native life he has afforded his readers. The description of the "Kachukia" dance of the Kikuyu is interesting, and one's only regret is that the author was not able to elucidate more details of its inner significance. The human aspect is, however, very vividly described.

The chapter of the folk ways of the Kavirondo is a valuable ethnological contribution, and the brief glimpse of the Butende is intriguing. One gathers that a considerable amount of the information recorded was gleaned from the Catholic missionaries who have worked in this region for a number of years, and may we hope that the scientific world will soon be given the benefit of their long continued

observations. There is beyond doubt a wealth of ethnological material in this area awaiting record by a trained observer who has the confidence of the people. If systematic enquiry is unduly delayed it will be lost, for the old generation of elders will soon all pass away and the rising generation with its veneer of mission teaching and the rudiments of a super-imposed education will prove an unreliable guide.

The growth of a passion for personal decoration among the Nilotic Kavirondo is a fact worthy of notice; the curious headdresses worn on ceremonial occasions have for long been known and the way they adapted hippo and wart-hog tusks, also the horns of the smaller antelopes, to their use has betokened considerable originality in the art of design. The Southern branch also evolved a type of feather headdress very like the Guardsman's bearskin. Of recent years, however, these people have developed the idea of using feathers to a most extravagant degree and have purchased large quantities from the ostrich farmers far to the east of their country. The result is astounding, as is well shown in a photo facing p. 174.

We gather the author, after leaving the Uganda Protectorate, proceeded westward through the Belgian Congo and, therefore, hope that shortly he will give us his recorded observations in that part of Africa; they cannot fail to be of interest both to the ethnologist and the general reader.

C. W. H.

Burma: Ethnography.

Milne.

The Home of an Eastern Clan: a Study of the Palaungs of the Shan States. 17
By Mrs. Leslie Milne, F.R.A.I., M.R.A.S. Oxford: Clarendon Press, 1924.
Pp. viii + 428; 20 Plates. 8 $\frac{3}{4}$ x 5 $\frac{3}{4}$.

The Palaungs are a somewhat simple hill people, living mainly in the Burmese Shan States. They are, however, Buddhists, having probably acquired their religion from their neighbours of a somewhat higher culture. They themselves have never troubled to reduce their language to writing, and such of them as are literate content themselves with books in the Shan language, which appears to be commonly known among the Palaungs. Evidently, therefore, we have here a mixture of cultural influences. But there seems to be much that is characteristic and original in the Palaung customs, and this work is to be welcomed as the first adequate record of them, especially as it is written in a very interesting and readable way.

After a brief account of their history, based on data which are unfortunately very scanty, the book deals in successive chapters with the life of the people from the cradle to the grave, giving a very full account of their customs, beliefs and folklore, including chapters on dreams, proverbs and riddles, and a number of folk-tales. Perhaps the most fascinating part is its detailed description of their elaborate etiquette of courtship and marriage. An Appendix gives particulars of local variations of custom (also especially in connection with marriage), and the book concludes with a full and useful index.

A frank indication of the author's method is given in a passage of her Introduction: "Nearly all that I have written in this book on the customs of the people has been told me by the Palaungs themselves." It must not, however, be inferred that the book consists merely of *ex parte* statements uncritically compiled. There is plenty of internal evidence that Mrs. Milne has carefully checked her sources, when it was possible for her to do so; and if her account of the Palaungs is a sympathetic and pleasing one, as indeed it is, that is no reason for doubting its substantial accuracy. She lived amongst these people for a considerable time and had ample opportunity of observing them at close quarters. Only those who do not know the Indo-Chinese races of the higher type will be tempted to think

that her picture of the Palaungs bears too roseate a hue to be a fair presentment of a living actuality.

I find few openings for criticism in the work. On p. 353 a footnote explaining that Sa-gya, the king of the spirits, is "possibly the same as Sakya" may possibly mislead an unwary reader. Sa-gya has nothing to do with Gautama Buddha (the Sakya sage, as he is styled) but is evidently, so far as his name goes, identical with Indra, who is often called by his other Indian appellation Śakra (in modern Burmese pronounced Thagya).

C. O. BLAGDEN.

CORRESPONDENCE.

Religion.

Hocart.

*To the Editor of MAN.***Divine Kings.**

SIR.—In my last letter on the subject of the Polite Plural (MAN, 1924, 128), I confined myself to that issue only. Will you now allow me to make a few remarks upon Mr. Rose's discussion of a totally different question—whether the early Greeks had divine kings or not?

18

Before discussing the matter, it is necessary, first of all, to realise that divine kingship is an elaborate system, and, secondly, to have a clear idea of that system. This condition is hard to fulfil; for whereas there are endless treatises on Buddhism, Christianity, and the other great religions, very little attention has been paid to the parent form of them all. So little is its importance understood by comparative historians that Sir James Frazer, after just touching the fringe of that religion, deserted it for objects of very secondary interest. Since no one else will collect the information from a comparative point of view, I have tried to make a beginning in a small work which is now only awaiting a publisher. Until then, I regret I must appear dogmatic, and affirm what I have no space to argue.

It must be remembered that divine kingship, just like its derivatives, has developed and changed, and one direction in which it has changed is towards a more thorough-paced doctrine of incarnation. It is this late stage which Mr. Rose has in mind and which prevents him from believing that the early Greeks had divine kings. It seems probable that, in its more archaic form, the term "incarnation" does not apply; the king is with the god and the god with him; he is divine but the identification is not as complete as later. The king must uphold law and order or the bond between him and the god is broken. In the words of Mr. Rose, "the 'gods love a virtuous king and reward him with material prosperity.'" Mr. Rose says Homer "merely" means that; I like the "merely," as if it was a small matter. Why should the gods love a virtuous king? That is the whole crux of the matter. Mr. Rose offers no explanation.

Mr. Rose seems to labour under the delusion that because a king in his divine character brings about material prosperity, therefore the people by their virtuous conduct cannot win the god's favour, and so secure prosperity; or, *vice versa*, that if the people's good conduct conduces to welfare, therefore the king cannot stand in a closer relation to the gods and be in a more especial manner the bestower of welfare. But, let us consider what is the connection between the king and the law: the king, as Homer says, *upholds* justice; and we find that idea permanently fixed by the Indians in the word *dharma*, from *dhr*, to support, Latin *fero*. The people by sinning alienate the deity; it is the king's function, therefore, not only to abstain from it himself, but to check it among his people, or atone for it by sacrifice on behalf of the people, because one single bad man can ruin a city. The fact that the whole people by being virtuous secure prosperity is so far from being

inconsistent with the king's divinity that it is almost its *raison d'être*.* With Mr. Rose's argument any one could prove that our forefathers never believed the king to be a Vicar of God on Earth; to the statements that kings are the Lord's anointed, to prayers that God may save him victorious and so replenish him with grace that he may always walk in God's way, he might oppose other statements that God protects the English people, and will always do so if the people put their trust in Him, and sin not; other prayers that He may deliver the people from their enemies, save the people and bless His inheritance, *and not take his Holy Spirit from them*—he might facetiously ask whether a whole country would be inhabited by Vicars of God on Earth.

Homer states clearly enough that kings stand in a special relation to the gods; else why does he call them δῖος, δῖος,†, divine; διοτρεφής, διογενής sprung from Zeus? Hesiod states definitely "the Gods are from Zeus"‡. If they were not, why were they retained to preside over sacrifices centuries after they had been stripped of secular power? Mr. Rose says having sacerdotal functions is "a very different thing from being himself divine." Is it? I have discussed the matter in my chapter on "Priests" in the little book referred to: perhaps the evidence collected there will induce Mr. Rose to reconsider this somewhat rash statement.

A. M. HOCART

Africa, South: Ethnography.

Wilman.

*To the Editor of MAN.***A Strange Milking Custom.**

SIR,—In connection with the article entitled "A Strange Milking Custom" (MAN, 1924, 31), it may interest your readers to know that illustrations of the Hottentots' similar custom may be found in A. Bogaert, "Historische Reizen door Asia," te Amsterdam, 1711, p. 99; and in P. Kolbe, "Beschryving van de Kaap de Guide Hoop," te Amsterdam, 1727, Vol. II, p. 35.

19

Mr. Graham Botha, Keeper of the Union Archives, informs me that some years ago he was told by a farmer in Bechuanaland that this custom was not unknown among the natives there.

Yours faithfully,
M. WILMAN.

McGregor Museum, Kimberley.

ANTHROPOLOGICAL NOTES.

Exhibition of Paintings of Anthropological Types from the Mt. Everest Expedition.—Mr. Francis Helps, A.R.B.A., who during the course of the last Everest Expedition had the opportunity of making a number of paintings and drawings of Lepchas, Tibetans and Sikkimese, is now exhibiting these at the Alpine Club Gallery (Mill Street, Conduit Street). Apart from the artistic quality of the work, the series is of considerable anthropological interest.

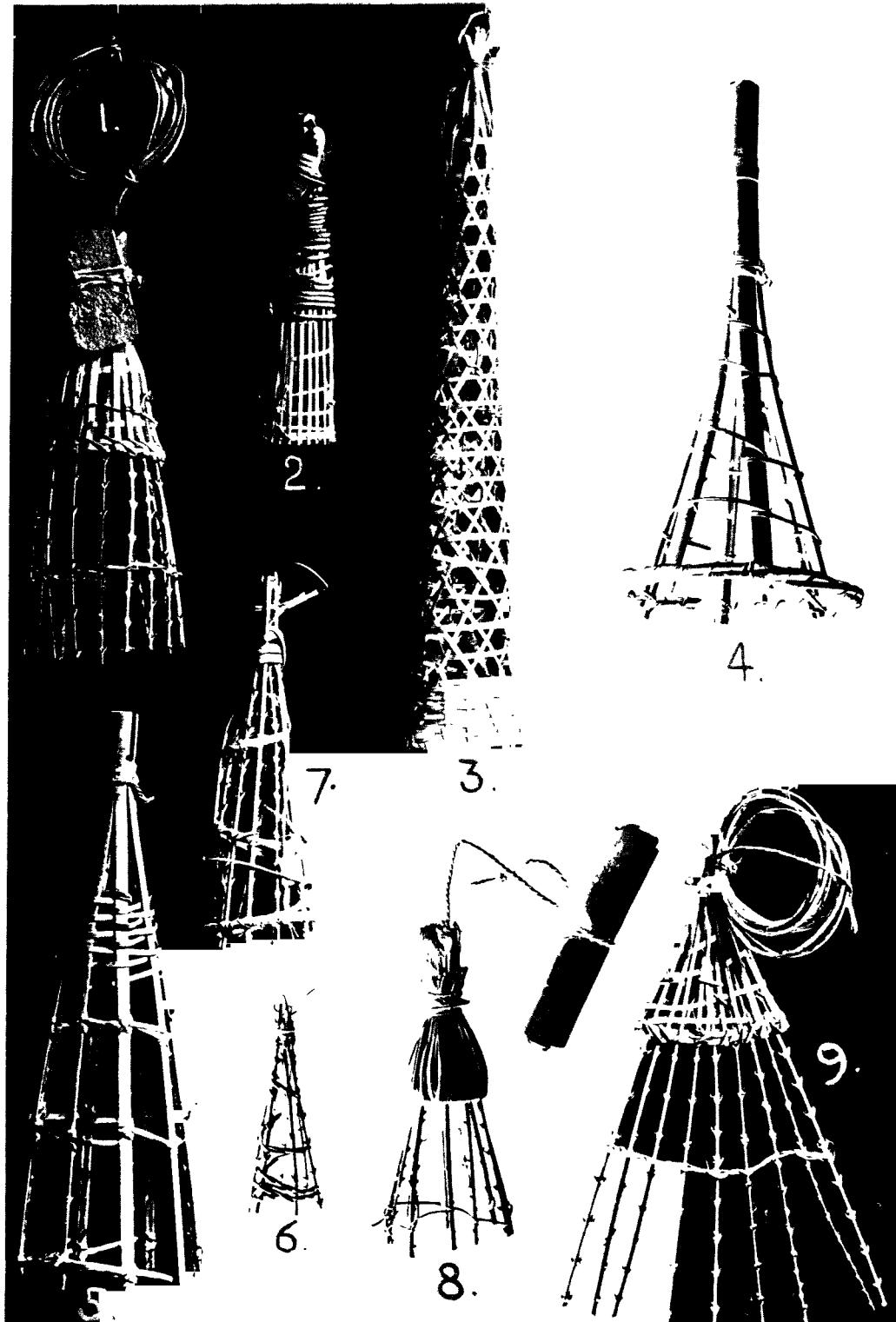
20

Errata in MAN, 1925, 1, p. 1, line 1, for "last year" read "in 1923"; p. 5, line 8 from bottom, for "torsoes" read "torsos."

* Cp. Kurudhammadjataka.

† Od. IV. 692 η τ' ἐστὶ δικῆ δείων ζασιλήσων: for such is the way of divine kings.

‡ The, 96.



THORN-LINED TRAPS FROM THE NAGA HILLS, MALAYA, INDONESIA,
MELANESIA, Etc.

ORIGINAL ARTICLES.

With Plate C.

Technology.

Balfour.

Thorn-lined Traps and their Distribution. By Henry Balfour, M.A.,
F.R.S. With Plate C.

21

I have been endeavouring to trace the geographical dispersal of a special and peculiar form of conical trap, which is usually employed for catching fish; since its distribution has a bearing upon one of the great dispersal routes of culture in the East. The principal feature of these traps is that the inside of the conical structure is lined with the thorns which beset the leaf-ribs, or *rachides*, of climbing-palms (*Calamus*, *Damonorops*, etc.)—the long slender palms which yield the rattan, or *rotang* cane.

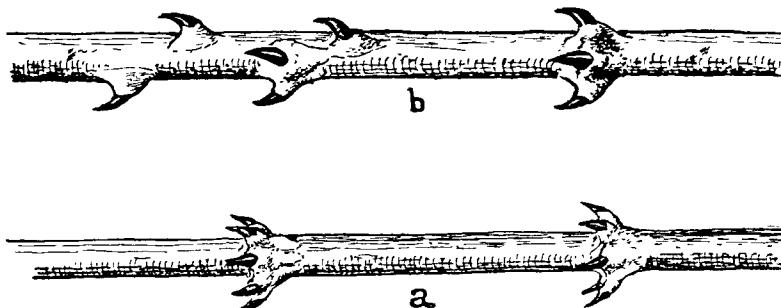


FIG. 1.—PORTIONS OF THE THORNY RACHIDES OF CLIMBING-PALMS (*Calamus* sp.).
a—Naga Hills. b—New Britain.

The grapnel-like thorns on the leaf-ribs are built into the traps so that their sharp points are directed towards the apex of the cone, rendering entry into the trap easy and exit impossible. In many cases the cone itself is built from these thorny *rachides*. A fish, attracted by the bait which is lodged inside the trap near its apex, can easily force its way into the trap, but is prevented from backing out again by the thorn-barbs, which grip it firmly.

Having already traced the dispersal of this type of trap throughout a wide though connected range, I was anxious to see whether I could find evidence of a still further extension of its distribution, and in 1922, during an ethnological tour in the Naga Hills of Eastern Assam, I made enquiries as to its possible use among the Nagas. Although there was at the time no record of the existence of this type in the Naga Hills, it seemed to be by no means impossible, in view of the marked Indonesian affinities exhibited by Naga culture. I was reminded of this line of enquiry when one day, while cautiously following the tracks of a wild elephant through dense jungle-growth in the Konyak country, I found myself completely held up by *calamus* thorns, from the embraces of which it was exceedingly difficult to get free. The reminder, however, if physically unpleasant, was ethnologically valuable. I cut off some of the thorn-bearing mid-ribs and took them back to camp on the Dikhu river. With the help of Mr. J. P. Mills I started enquiries as to whether the Nagas ever made any use of these natural grapnels, avoiding any direct reference to traps and any suggestion as to what I was seeking. At first the replies were negative, and I was given to understand that the thorns served no useful purpose. Later on, while camping on the Doiyang river in the Lhota country, a very intelligent Liye-Lhota Naga, from Okotso village, volunteered the information that these *calamus* ribs were used by his people, but that the practice was almost obsolete. When asked what was done

with them, he said, to my great interest, that a special kind of fish-trap was made with them. A day or two later he returned from Okotso with a trap which is practically identical with forms which range through the Malayan and Indonesian area and extend as far as Melanesia. This Lhota trap, the first to be recorded from the Naga Hills, is shown in Pl. C, fig. 2. It consists of an openwork conical structure of bamboo, formed by splitting the bamboo into rays as far as a node, which remains solid and forms the apex of the cone. The rays diverge toward the open base and are bound round with thin bamboo strips. On the inside of the hollow cone are lashed equidistantly four thorn-bearing *calamus* ribs, with the points of the thorns directed inwards. A stone sinker and a long retrieving-line of creeper-stem are attached to the apex. In use, the trap would be baited inside with a fresh-water crab, placed far up the cone, and it would then be sunk in a river-pool, the free-end of the line being retained by the fisherman, or attached to the bank. A fish, lured by the bait, enters the trap, but cannot retreat as the thorns prevent any backward movement; in fact, the trapped fish can hardly move at all. The trap is drawn ashore by means of the line, and as the fish cannot be drawn out, is partly dismantled to open it out. The discovery of this type of trap in the Naga Hills, extending as it does the geographical range northward and inland, interested me greatly, and I begged Mr. Mills to prosecute further enquiries after my departure. This he has very successfully done, and I have, through his kindness, received several varieties of similar traps from other Naga tribes.

Two traps obtained from the Chang Nagas of Yongiemi village (Pl. C, Fig. 1) differ from the above-mentioned Lhota example in their more open structure. The *calamus* ribs are at one end set in a rigid bamboo cone, formed as in the last, but project freely below for about 6 inches, being kept in place by a light girdling of cane-strip, giving great flexibility. Stone-sinker and retrieving-line, and also the mode of use, as in the Lhota example. This trap is about 14 inches long. Mr. Mills has also sent a couple of traps of the same construction as Fig. 1, 18½ and 19½ inches long, procured from the Sangtan Nagas of Alisoporr village; and I brought home a larger Sangtam example of similar type, 27 inches long, with an opening 8 or 9 inches wide. In these, the bamboo cone is longer in proportion than in those from the Chang tribe. Fig. 3 shows a trap, obtained from the Konyak Nagas of Shiong, somewhat differently constructed and used for catching rats. The narrow cone, 23 inches long, is constructed of open-work basketry, the inside being lined with four equidistant thorny *calamus* mid-ribs (cf. Fig. 2). This trap is placed over a rat-hole and the animal is smoked out and caused to bolt into the trap, where it is held fast by the recurring thorns. In Fig. 4 is seen another Naga variant. A wide-open cone, 17 inches long, is formed by splitting a length of bamboo into six stout rays, each of which has a *calamus* rib attached to it inside. The rays are kept apart by a stout ring of creeper-stem. The solid bamboo at the apex serves as a handle. This implement is used by the Southern, or Tukomi, Sangtams of Sangpurr (Thachumi) as a scoop for hand-catching fish which have been stupefied by poisoning the water. The above represent the varieties of this trap in the Naga Hills, as far as is known at present.

To turn now to the geographical dispersal of similar traps elsewhere.

On my return to Calcutta from the Naga Hills, I noticed in the Indian Museum a couple of traps of this kind which had been collected by Major W. R. Stone on the Pichoung River, near its junction with the Koladan River, in North Arakan, Western Burma. It is used by the Chins, who call it *wechun* (Fig. 5). In construction it resembles Fig. 4. The cone, 20 inches long, is formed of bamboo split as before, the rays being braced with cane at intervals, and each bearing along its inner surface the barbed *rachis* of a *calamus* or *daemnorops*. The example figured was given me for the Pitt-Rivers Museum. The other is figured and described by Mr. I. H. Burkhill

in the *Journ. Asiat. Soc. Bengal*, N.S., III, 1907, p. 441. These traps are baited and are set in a rapid with the opening downstream; a stone, attached by a short line, serves as sinker, and a long retrieving-line of rattan fixed to the trap is attached to a bamboo stake stuck in the river bank. A very small trap, $6\frac{1}{4}$ inches long (Fig. 6), constructed entirely of *calamus* ribs bound together to form an open-work cone, is one of two sent to the Pitt Rivers Museum by Mr. H. C. Robinson. These were made by Malays, and are used by both Malays and Sakais on the Perak River, Malay Peninsula. An identical example is in the Malay series in the Münster Museum, Bâle. The local name is *tengkalak onak*. I saw, in 1914, in the Museum at Kuala Lumpur, a long, narrow variant, called *tuar*, which is used for trapping fish in the rice-fields. One may gather from Skeat and Blagdon ("Pagan Races of the Malay Peninsula," I, pp. 223 and 419) that the Benua-Jakun of Johor and the Semang of Kedah employ such *calamus*-rib traps. R. O. Winstedt ("Malay Industries," part II, 1911, p. 15) mentions Malay traps of bamboo with barbed spikes or thorns; and he also (p. 37) refers to pigs being "caught by means of a hollow conical frame with ribs of downward turning thorns; pushing his snout in, the pig cannot withdraw it on account of the lining of thorns, and so runs about blindfolded in agony." The trap is baited. It is clearly of the same type as those used in fishing.

Examples of this trap have been collected in Sumatra. A long, narrow conical example, *hinar*, nearly 35 inches long, formed of bamboo strips and *calamus* midribs spirally wound round, is included in the F. W. K. Müller collection ("Verof. a. d. Kgl. Mus. f. Völkerkunde," Berlin, III, p. 53, Fig. 147). It was obtained from the Karo Battaks. Another, from Kota Pinang, in the Battak country, is described by H. W. Fischer ("Kat. d. Ethn. Reichsmus.," Leiden, VIII, p. 151, No. 370), and is also cone-shaped and set with *calamus* thorns; used baited and placed in a river; length, 10 inches; width, $2\frac{1}{4}$ inches. From Central Sumatra comes an example, *una* or *unak*, in the Leiden Museum: a conical structure of thorn-bearing *calamus* ribs, $13\frac{3}{4}$ inches long and $3\frac{1}{8}$ inches wide, bound spirally with cane and fitted with a stone sinker. It is set in a river with the opening downstream (H. W. Fischer, *op. cit.*, X, p. 83). A similar trap, *oenah*, from the Rawas River, Palembang district, South Central Sumatra, is figured and described by Van Hasselt ("Ethn. Atlas van Midden Sumatra," 1881, part ii, pl. 126, Fig. 4, and p. 62). Another example, *unaq*, conical, and made from whole or split thorny *calamus* ribs, bound together and furnished with a long retrieving-line, is described by Fischer (*op. cit.*, XII, p. 122) as used only in river-fishing. In the Vienna Museum is a similarly-constructed specimen, *kori*, from the Lampongs district of South Sumatra.

Varieties of this trap from Billiton Island, off the south coast of Sumatra, are listed by Fischer (*op. cit.*, IV, pp. 156, 157) under the names *unak*, *tetapuk* and *tengkalak*, as used in river-fishing. Some of these appear to be built up solely from *calamus* ribs; others appear to resemble the Arakan examples (Fig. 5), the conical structure being of bamboo split into rods and lined with the thorny *rachides*.

Passing eastwards, we find these traps again in Borneo. I. H. N. Evans ("Among Primitive Peoples in Borneo," 1922, p. 109) mentions the use by the Dusuns of bottomless conical baskets of rattan twigs, the thorns of which point towards the apex of the cone. These are set in small gaps in a stone wall built across a stream, the mouths of the traps facing up-stream. Similar traps, *antapuk*, used by the Sambas and Sekajam Dayaks of West Borneo, are described by H. H. Juynboll ("Kat. d. Ethn. Reichsmus.," Leiden, I, 1910, p. 191).

I have at present no references to thorn-traps from the Moluccas and other groups in the eastern Malay Archipelago, but they reappear in North Luzon, Philippine Islands, among the Negritos of Pampanga. These are described by H. W. Walker ("Wanderings among South Sea Savages," 1910, p. 78) as long, cone-like objects, the insides lined with thorn-bearing rattan ribs. A northward extension is manifested

by the use of thorn-lined fish-traps among the Ami coastal tribes of Formosa (Mrs. McGovern, "Headhunters of Formosa," 1922, p. 184).

In New Guinea the dispersal is a wide one. Reference to small thorn-lined traps used for fishing in the late German Territory (? the Sepik R.) is made by Lorenz ("Nova Guinea," 1913). W. N. Beaver ("Ann. Rep. on Papua," 1914-15, p. 196) illustrates the making of conical thorn-barbed fish-traps, *sirawa*, among the Orokava tribe of the coast and inland, between the Yodda Valley and the old Papuan boundary. The traps are inserted in gaps in a dam built across a stream or creek. Fig. 7 shows a small trap, 12 in. by 3½ in., closely resembling the smaller Malayan example (Fig. 6) in structure, and consisting simply of *calamus* ribs bound together spirally with cane strip to form a hollow cone. This was obtained by Mr. Rohu at Duvira Bay, between the Mambari and Gira rivers, in N.E. British New Guinea, and is now in the Pitt Rivers Museum. When in use, these traps are baited and are concealed in a covering of leaves. In the Fly River district such traps appear to be widely used. Examples, *karo*, were obtained by the Rev. J. Chalmers from the mouth of the Fly, composed entirely of *calamus* ribs united to form a hollow cone. One of these is in the British Museum (Partington, "Album," II, pl. 194), and two specimens in the Pitt Rivers Museum are probably from the same source. Another example, also from the Fly River, is in the Museum at Sydney. This type is recorded from the Tedi R. (Alice R.), a tributary of the Fly, by L. Austin (*Geogr. Journ.*, LXII, 1923, p. 345); and W. N. Beaver ("Unexplored New Guinea") described the same type from the Bamu, another tributary of the same river. Other New Guinea varieties, of which I do not know the precise localities, are to be seen in the museums at Cambridge, Sydney, Brisbane and elsewhere. Some of these are very obtusely conical with proportionately wide openings, and must be intended for some special kind of fish. Partington ("Album," III, 90, Nos. 5 and 6) figures two specimens, one of which, said to be from New Guinea, closely resembles a New Britain type (cf. Fig. 8), having a light wooden float attached to the apex by a short string.

The New Britain Archipelago furnishes other examples. The one shown in Fig. 8 was collected by the Rev. George Brown in New Britain, and was given by him to me. It is called *ugut* and is 10 inches long and 4 inches wide. In this the *calamus* ribs are fixed in a conical cap of folded palm-leaf, gathered together at the apex, the thorny rods standing free below. It is fastened to a light wooden float by a short cord. Brown refers to these traps in "Melanesians and Polynesians," 1910, p. 323. The use of the float is well indicated in a figure of one of these traps from Sandwich Island (Tiaul), close to New Ireland (Biro Lajos, "Daten zum Schiffahrt und Fischerei d. Bismarck-Insulaner," 1905, pl. i, pp. 71, 72). The trap, baited with a mussel, hangs suspended from the float with the opening downwards, in a gap in a coral-reef, and a long line from the lower end is attached to a stick stuck in the reef. The trap, called *kabane*, is made from rattan ribs, called *uje*; and the fish must attack the bait from below. A figure of one of these traps from the New Britain Archipelago is given by Von Luschan in "Beiträge z. Völkerkunde," pl. XXXV. It is of the same type as that shown in Fig. 8. Partington ("Album," III, 37, No. 6) also figures one in which the cone is of bark-cloth, instead of palm-leaf. A long, narrow conical trap of this kind, of the type shown in Fig. 7, is figured by Powell ("Wanderings in a Wild Country," p. 177). In this instance a very small float is attached by a long line.

From the Solomon Islands there is a specimen, collected from Choiseul natives at Gizo, in the Pitt Rivers Museum (formerly in the Partington collection). It is 8½ inches long and 4 inches wide, and of the same construction as Fig. 6. Another Choiseul specimen is in the British Museum, and a third, fitted with a wooden float, like Fig. 8, is in the Dresden Museum. A specimen, also of the "Malay" type, obtained at Kumbukotta, Ronongo, Solomon Islands, and one from Bougainville Island, are in the Melbourne Museum. Lastly, there is in the Pitt Rivers Museum a trap, said to be

from the Santa Cruz group, constructed exactly like the New Britain type (Fig. 8), but with the light-wood float fixed through the apex of the palm-leaf cone.

The geographical dispersal of this peculiar type of trap is of some ethnological importance, since, owing to its almost completely linked-up distribution, it affords one of the most convincing chains of concrete evidence helping to prove an affinity between the culture of the Naga Hills at one end of the range, and Melanesian culture at the other end. The place of origin of this trap is uncertain, but it cannot be doubted that all these thorn-lined traps are referable to a common prototype and form a connected series. The line of dispersal of these, coupled with that of the "Indonesian" loom, the fishing-kite, the use of a flexible thong in frictional fire-making, and a number of other items, helps to indicate the general line of culture-dispersal eastwards into Melanesia. The main route appears to have passed to the north of New Guinea, avoiding the Torres Straits route, and reaching the Melanesian Islands from the north-west. The influx of Melanesians into the South-eastern area of New Guinea introduced many Melanesian culture-elements and it is probable that the thorn-lined traps may have thus been diffused there. The fact of these traps being common on the Fly River and its tributaries is rather puzzling, as this district lies far outside the area which is dominated by the Papuo-Melanesian culture, which hardly extends westward beyond Cape Possession. Unless this trap reached the Fly River *via* the southern route from the Arafura Sea, we must, perhaps, assume that coastal trading and even raiding were the chief factors in spreading the knowledge of a type of fishing-trap whose efficiency is at once obvious and the necessary materials for the making of which are readily available over a wide area. These are broad questions outside the scope of a short paper.

Before concluding, I wish to refer to an interesting fishing appliance which was discovered by Mr. J. P. Mills in the Konyak village of Kamahu, in the course of his following up my enquiries regarding the distribution of these traps in the Naga Hills. This object, one of two sent to me, is shown in Fig. 9. Its structure and triangular form suggests exactly a trap of the type shown in Fig. 1, reduced to two-dimensional form. It consists of nine *calamus* ribs, their free ends spreading out like the fingers of an open hand, the other ends being gathered together and fixed in a flat textile triangular framework of split bamboo, the apex of the triangle being the unsplit end of the bamboo. This "frame" is in two layers, between which is enclosed a stone sinker. The points of the thorn-barbs are directed towards the apex, to which is attached a long retrieving-line of creeper-stem. The Konyaks are bad swimmers and cannot dive; and, after poisoning the water, they cast in these fish-drags and draw them to the bank by means of the hand-line. In this way, with the help of the grapnel-hooks, the half-stupefied fish are pulled out. The length of this fish drag is 16 inches and the width 10½ inches. It is obviously a specialised variant of the conical traps, but I do not know of this two-dimensional retrieving-drag from elsewhere.

HENRY BALFOUR.

Ethnology.

Perry.

Pearls and Pearl-Shell in the Pacific. By W. J. Perry, M.A.

22

In MAN. 1924, 131, Dr. Haddon has set out to examine Professor Elliot Smith's generalisation concerning "Givers of Life," with specific reference to pearls. Not only do the importance of the topic and the manner of the criticism demand some reply, but the total misunderstanding of the position on the part of the writer who summarised Dr. Haddon's communication in *Nature* (December 20th, 1924) make it imperative that the situation should be cleared up. In offering some comments on Dr. Haddon's communication, I shall take the sequence adopted in "The Children of the Sun," and shall speak of distributions first, and secondly of

the possible significance of pearls and pearl-shell to the people of the archaic civilisation.

Dr. Haddon's treatment of the distribution problem is to distinguish sharply between pearls and pearl-shell—"I propose here to limit myself to the subject of "pearls"—and thus to reach the conclusion that—"Mr. Perry overstated his case" when he wrote, 'That the Polynesians of old were seeking pearls stands beyond "doubt"—thereby leading the reader to conclude that I had ignored pearl-shell in my discussions, and thus entirely confusing the issue. I cannot, however, absolve myself from blame in this matter, for I have on several occasions used the word "pearls" when I should have said "pearls and/or pearl-shell." But there is not the slightest doubt from the expressions that I used in my work that I coupled pearls and pearl-shell together for my purpose. Whether the pearl-shell itself or its occasional content was sought after is a matter of secondary importance, especially from the point of view of the distribution problem. One can hardly search for pearls without looking for pearl-shell. I claim that the people of the archaic civilisation were influenced to a large degree in Oceania in their choice of settlement by the presence or absence of pearl-shell, and from that point of view it does not matter materially whether they sought pearls or pearl-shell or both. I may be doing Dr. Haddon an injustice, but I do not think that he has once mentioned that I speak indifferently of pearls or of pearl-shell when speaking of distributions. The quotation he gives from my book makes my meaning quite clear, and other similar statements can be found in my writings. It is, therefore, evident that he is raising a false issue, in that his statements lead the unwary to conclude that I rested my case exclusively on pearls, which is not so. Dr. Haddon will find that Elliot Smith, J. W. Jackson, Kunz and Stevenson, as well as the "Oxford Economic Atlas" (which was the foundation of my work on geographical distribution) group pearls and pearl-shell together, so I am in good company. But although Dr. Haddon, in distinguishing between pearls and pearl-shell for the purpose of discussing my views regarding the origin and spread of the archaic civilisation, is confusing the issue, yet he cannot compete in this respect with Professor Peet. This authority on Egyptian archaeology, in his endeavours to discredit my views (see *Journal of Egyptian Archaeology*, Vol. X, pp. 63 seq.), accuses me of using, when I discuss distributions, any substance that comes to hand—as if I were responsible for the needs and desires of these people—"If it is not gold it is "the pearl-oyster, and if the latter is not to be found the pearl-mussel will serve his "turn." To such a pass does hostility to a theory bring a man that he is unable to realise that the people of the archaic civilisation were not systematic zoologists, and that they did not bother whence came the pearls or pearl-shell that they used. This is a particularly pointless criticism, in that the Polynesians used the shell of the oyster, while the Mound-builders of the United States used the pearl-mussel. Such criticism as this comes very near to the farcical.

Having now made it clear that, from my point of view, it is a matter of secondary importance whether pearls or pearl-shell were sought, so that it is logically inadmissible to detach them from each other. I should now like to ask Dr. Haddon whether he accepts my conclusions concerning the distribution of the settlements of the archaic civilisation? If he does not accept them, what alternative explanation has he of the facts? He mentions that Torres Straits has no megalithic monuments and no terraced irrigation, in spite of the fact that the region contains plenty of pearl-shell. That may be so, but what has he to say of the carved stone images and of the mummification of that region? The region of New Guinea has megalithic monuments and irrigation, in the neighbourhood of gold-fields and pearl-beds, so the presence of the people of the archaic civilisation is fairly certain, especially as we know that they were working the gold-fields. In the case of Torres Straits there is undoubtedly some local reason why the whole of the culture was not adopted

but what is there shows that there is no doubt that its influence was felt. But why does Dr. Haddon, who says that the matter must be "fully discussed," confine his remarks in such connections to instances that seem to cause the theory to break down? Has he any other explanation for the distribution of the settlements in New Guinea, or, for that matter, of the stone images of Torres Straits, not to speak of the mummification? Can he account for the working of gold in a region where the natives are ignorant of it? Why does he not discuss the case of the island clusters of the Gilberts, Ellice, Tokelau, *et cetera*, where no pearl-shell is found, and all of them colonised later from other groups? Such a discussion would be fruitful. Can Dr. Haddon explain why the people of the archaic civilisation ignored these groups and went on to Tahiti and the Paumotus, for instance? I have given my explanation, which fits the facts, and I should like to see it "fully discussed," not evaded by the forensic device of choosing out particular localities that seem to offer difficulties. Dr. Haddon has his own tentative solution of the problem. He is of the opinion that "it is likely that a material so easy to obtain and so easy to work would early have engaged their attention." He entirely ignores the fact that the culture of the people in the pearl-shell areas differed markedly from that of islands elsewhere. Is he prepared to argue that the Tahitians, for instance, started using pearl-shell for the reason he gives, and then proceeded to build stone *marae*, to carve stone statues, to elaborate the dual organisation, to invent the sun-cult, to practise mummification, while the people of the Gilberts lagged far behind? I take it that he will not be anxious to undertake this task. On the other hand, the assumption that the people with this culture settled especially where they found pearl-shell fits the facts.

I now pass on to consider Dr. Haddon's remarks concerning "Givers of Life." He brings up two eminent authorities to refute Dr. Mingana, from whom came the information that the ancient Persian word for pearl, *marjān*, meant "Giver of Life." Both of Dr. Haddon's experts seem to have overlooked the adjective "ancient," and lay stress on *modern Arabic*, instead of *ancient Persian*. In passing I must refer to the writer who summarised Dr. Haddon's communication in *Nature* (December 20th, 1924, p. 909). He said: "Prof. Elliot Smith founded his argument upon the word 'Margān,' which he stated meant in Ancient Persian 'Pearl' and 'giver of life'; whereas it means 'coral.'" This is not so. Elliot Smith's argument was founded on entirely different lines of evidence, from the facts connected with cowries and other shells. Indeed, it was only after he had called attention in public lectures to archæological evidence suggesting the belief among the ancients in the pearl's magical potency as a "giver of life," that Dr. Mingana thanked him for solving a philological problem which had been puzzling him for some time, why the earliest word he could discover for pearl actually meant "giver of life." The note included in "The Evolution of the Dragon" was thus not the basis in which he rested his argument, but merely a curious and unexpected confirmation.

Dr. Mingana has replied to Dr. Haddon's authorities on the linguistic points, and the matter must be left for discussion between the experts. But I cannot refrain from commenting upon Dr. Haddon's unkind treatment of the distinguished scholars who helped him to fashion his indictment. Cambridge enjoys the luxury of two Professors of Arabic, and Dr. Haddon quotes the opinions of both of them, presumably without letting either see his colleague's statement. The Sir T. Adam's Professor informed him that "*marjān* means 'coral,' not 'pearl.'" Moreover, he says: "It has nothing to do with the words for pearl—*magarites* (Greek) *et cetera* : but he qualified these categorical statements with the admission that 'Dozy says that *marjān* originally meant 'pearl' though now always 'coral'; he appears to derive it from the Greek *margarites*.'" Had Dr. Haddon been content with the opinion of one Professor of Arabic a serious difficulty might have had to be met. But fortunately for us he naively puts into juxtaposition the views of

The Lord Almoner's Professor of Arabic : "As for the Arabic *marjān*, I think that 'Dozy is no doubt right in saying that it is derived from the Aramaic *marganitha*, 'pearl,' which is itself derived from the Greek *margarites*. The Arabic lexicographers usually explain *marjān* as meaning 'small pearls.' " Perhaps the adjective "small," once used by the nurse of Captain Marryat's hero in her famous excuse, might be some extenuation for the Sir T. Adam's Professor when he considers his emphatic denials of the meaning "pearl."

In discussing the ideas attached to pearls and pearl-shell, Dr. Haddon allows himself full liberty of choice of place in his attack, but evidently wishes to limit me to the Pacific in my reply. He discusses the text from the "Atharva Veda," which is entirely in my favour, and then asks for evidence that pearl-shell had any magico-religious significance in Oceania. Does this mean that he believes that Asiatic ideas were devoid of influence in the Pacific? Incidentally, he answers his own question, and thus sweeps away the great part of his argument; but I propose to take the point up, for it is important. It is quite conceivable that the original significance of pearl-shell may have become lost, just as has happened in the case of gold in New Guinea. Such a happening would leave our position untouched. Dr. Haddon presumably pays no attention whatever to the possibility of degradation. In this he is like many other anthropologists, who, in their desire to demonstrate advance in all places, entirely overlook the possibility of the opposite process having occurred. There is no doubt that the people of India at an early date regarded pearls and pearl-shell as "givers of life." So do the Chinese. So did the peoples of North America. Thus the Pacific was encircled by peoples who attached a magical value to pearls and pearl-shell. This certainly means that the belief in pearls and, or, pearl-shell as a "giver of life" was held by the people of the archaic civilisation, and, if the unity of this civilisation be accepted, it means, moreover, that it was carried about the world. As I have argued at great length in "The Children of the Sun," degradation has played a very important part in the histories of the communities of Oceania, and it is possible, even probable, that so highly artificial a belief as that in the life-giving powers of pearls and of pearl-shell can have disappeared. I have not yet found a single instance in which a critic of the general theory has appreciated the principle of the action of degradation. If they had, they would have seen that many of their objections fall at once to the ground.

Fortunately, it is possible to show exactly how this degradation can have come about in the case of pearl-shell. Dr. Haddon "would like to see evidence adduced "that (pearl-shell) was regarded in Oceania as possessing any magico-religious "significance." He answers his own question by quoting Dr. C. E. Fox's "The Threshold of the Pacific," where it is definitely asserted that pearl-shell is a "giver of life" in San Cristoval. Dr. Fox says that the class of sacred chiefs in San Cristoval "set great store by the pearl-shell and mother-of-pearl." (363). Moreover, "mother-of-pearl, *dahi*, has a certain sacredness" (253). The sacred chiefs have the privilege of wearing crescentic ornaments of mother-of-pearl, which also are called *dahi*, or *warowaro* (297). In this island, therefore, pearl-shell is sacred: it is associated with sacred chiefs, and these sacred chiefs are themselves culturally associated with the archaic civilisation, which civilisation, as I have urged, included a love of pearls and of pearl-shell, not necessarily, I may mention here, of both, but certainly of pearl-shell. Amulets of pearl-shell are made in other islands of the Solomons (cf., for instance, C. Ribbe, "Zwei Jahre unter den Kannibalen der Salomo-Inseln," pp. 236, 237, 264). In British New Guinea clam pearls are used in magic, as also is the clam shell itself. There is thus no doubt as to the answer to be given to Dr. Haddon.

I should like, however, to go further, and to show how this sacredness can be lost. In Torres Straits there is a correspondence with the Solomons in the fact

that pearl-shell and crescentic ornaments are named alike, and a difference in attitude towards this ornament. For in Torres Straits the word for crescentic pearl-shell ornament is *mai*, which is also the word for pearl-shell. The Torres Straits men use pearl-shell ornaments when on the war-path. "When fighting the " Miriam wore a whole trimmed pearl-shell, *komerkomer mai*, on the chest. Mr. " Wilkin was informed that the Mabuiag men on such occasions wore pearl-shell " ornaments, *mai*, on their chests, inscribed by means of sharks' teeth with the " owners' totem. In all the drawings of warriors by natives a large deep crescentic " *mai* is indicated." (IV, 204). Yet we are told by Dr. Haddon that "various " ornaments, such as the crescentic, pearl-shell chest-pendant and the deformed " boar's tusk of the Bomai Malu ceremonies may have had a magical import at " one time, but we did not discover anything to lead us to regard them as having " any special significance at the present day." (*Op. cit.*, 193). It is important to note that, as in San Cristoval, the crescentic pearl-shell ornaments are called by a name which is applied to pearl-shell itself, but with this difference, that in Torres Straits they evidently are now not magical in nature. If that be so, an interesting problem is presented, namely, which is the older usage, that of the Solomons or that of Torres Straits? As Dr. Haddon has thrown at me the absence of terraced irrigation and of megaliths in connection with this region, I cheerfully use this weapon with which to defend myself, and urge that the less-developed nature of the civilisation of Torres Straits suggests a less powerful dose of culture from the archaic civilisation than that received in San Cristoval, with its sacred chiefs, mummification, pig-tailed statues, dolmens and so forth. I claim that the San Cristoval attitude towards the pearl-shell is the original attitude, and that all such ornaments were used in the first instance as amulets. This gives a meaning to the Torres Straits practice which otherwise it lacks. It also gives point to my statements, made earlier in this communication, that piecemeal objections are often entirely beside the point: they are not objections, but simply minor details of local development that need explanation by local conditions. The comparison between San Cristoval and Torres Straits well illustrated the process of degradation, or simplification if you prefer, of culture. The original significance of an object may in time become simply ceremonial or purely ornamental. There is no doubt that the people of the archaic civilisation regarded pearl-shell as a "giver of life," and their settlements in Oceania were largely determined by their love for that substance. The attribution of life-giving qualities to pearl-shell is obviously the outcome of a long process of theorising that well may have taken place somewhere far remote from Oceania. So when the original culture with this belief began to break up, as it evidently did in Torres Straits, the belief concerning pearl-shell tended to go too, simply because the belief had lost its logical basis. This instance shows that the process of degradation must very carefully be taken into account by those who wish to discuss questions of transmission of culture, and especially when they try, by quoting certain selected facts, to damage a theory which must be discussed in its entirety. That is the only sort of "full discussion" which is logically permissible.

W. J. PERRY.

Ethnology.

Mingana.

On the Meaning of the Persian Word for Pearl and Coral. By **23**
A. Mingana.

My attention having been called to the criticisms of Professors Browne and Bevan concerning my derivation of the Iranian word *marjān*, for pearl or coral. I should like to make the following observations.

(1) I believe it would be unreasonable to refuse to see an intimate linguistic connection between the Aramaeo-Syriac *marganitha* "pearl," the Graeco-Latin *margarites* (do.), the Turanian *marjan* ("pearl, coral") and the Arabic-Persian *marjān* (do.). To appeal to Arab lexicographers for the right meaning and derivation

of a foreign Arabic word would only be a little better than to appeal, say, to a modern Moroccan for the right meaning and derivation of a Spanish loan word.

(2) I do not believe that a single Greek or Latin scholar has seriously contended that the word belonged etymologically to the language of Homer or of Cicero; indeed, many obvious reasons seem to militate against such a contention. I can also assert with confidence that the word is not Semitic in origin: it is certainly foreign to the taste of all the Semitic groups of dialects: and I should have but little esteem for the philological knowledge of anyone who might be tempted to resort for its right explanation, in its present form, to the Turanian dialects of Central and Eastern Asia. Of Chinese I know nothing. The only alternative left to us is the Iranian branch of languages of Western Asia, that is, the languages of those peoples who, by their proximity to the Persian Gulf, might have known something about the nature of pearls.

(3) I believe, therefore, that the word *marjan* is composed of two Persian vocables: *mard* and *jān* (or *gān*). The first word is used in compounds in the sense of the Arabic *dhu* and Aramaic *mar*: "man of," "owner of," "possessor of," "having for quality." The second word *jan*, *gan* (in Zend, *yan*) is the common name for "soul," "life," and needs no explanation. For the sake of euphony or for other reasons it happens in many languages that the dental *d* interchanges with *t* or is completely eliminated. This phenomenon has happened in the present case, so that really the word *mar-gan* stands for *mard-gan* and means literally "owner, etc., of life." It would be useful here to remark that this very Persian root *mard* has been used in other linguistic compounds which, like *mard-jan*, have invaded several civilised languages of the globe. In this category is to be counted *mard-vn-giah* (man-like plant), "mandragora, mandrake." How deeply has the Persian language influenced Arabic and many ancient languages of Europe may be seen in the recent work of the late A. Scher entitled: "Mots Arabes dérivés du Persan." To illustrate the frequency of the elimination of the letter *d* in the Iranian languages we will here draw attention only to the indisputable fact of its disappearance in Kurdish, Zaza, and Bactrian, in the word *mad-ah* which plays exactly the same rôle in the gender of nouns as that of our English "she," ex. gr. *ma-kar*, "she-ass" for *madah-kar*, etc.

(4) The Aramaeans, those wonderful inhabitants of a country which has always formed the natural link between East and West—took the word *mar-jan* (as they took scores of other words) from the Iranians, their neighbours, and brought it to the Greeks with the suffix *itā*: the distinctive and unmistakeable feminine termination of their language. From the Greeks the word spread to the rest of Europe.

(5) Absolute certainty is always good, but in philological derivations of uncommon words it is a very rare bird. In linguistic phenomena of this nature we are generally satisfied with a working probability, and in some difficult cases we are even contented with a plausible hypothesis that appears to explain in a better way all the difficulties inherent in the words. Such a plausible hypothesis we venture to claim for the derivation we have given to the word under consideration. A better derivation is not impossible in the future; but the future is beyond the power and the ken of the present.

A. MINGANA.

Africa, West: Albinism.

Migeod.

Albinism at Mori on the Gold Coast and Elsewhere. By F. W. H. Migeod.

24

With reference to MAN, 1924, 121, on the "White Indians" of Central America: there are places in Africa where there is a local tendency to Albinism. One such I knew was Mori, about 5 miles east of Cape Coast Castle, now a small

fishing village. I used to visit it occasionally from Cape Coast Castle some twenty years ago, and was struck by the large number of albino children in the place. If I remember rightly, I must have seen five there on one occasion alone; but I never saw an adult. These children would, if still alive, be grown up now; but if there be a local tendency to albinism, there will have been other albino children born since.

Mori contains the ruin of an old Dutch Fort, which was important in the old days. It never, however, had the importance of Elmina, 8 miles west of Cape Coast Castle, which was inhabited, first by Portuguese, then by Dutch from the fifteenth century down to 1871 or 2, when the Dutch handed it over to the English.

The albinism cannot be due to European blood in any way, for in Elmina, though that place shows European descent in the lighter skins of many of the inhabitants and in their features to some extent, there is no tendency to albinism that I am aware of. Nor is there at Cape Coast Castle, which has always been English, except for a few short years when De Ruyter captured it.

The fact remains, therefore, that at Mori there is, or was, a local predisposition to a failure of the black pigmentation.

Single cases of albinism are, of course, common everywhere, but I have never seen an elderly adult. All have been children or young men, and females I have not seen that I can recall.

I might here mention the peculiar case of a soldier of the Gold Coast Regiment. He was either a Dagomba or a Dagarti, both tribes located in the Northern Territories of the Gold Coast Colony. He fell sick, and when, after a long time, he returned to duty, he had become entirely white. The black pigmentation had disappeared, except for some freckles. He was in this state when I last saw him, as a corporal, in about 1916.

The natives of Africa in out-of-the-way places will account for albinism by saying the mother when pregnant saw a white man, and, owing to the shock, brought forth a white child.

My notes on this subject are at home, and as I am at present travelling in Sierra Leone, I am not perhaps able to add all the detail I might have had I been in England.

F. W. H. MIGEOD.

REVIEWS.

Sociology.

Rivers.

Social Organization. By W. H. R. Rivers. D.Sc., F.R.S., etc. Edited by W. J. Perry, M.A. London : Kegan Paul. 1924. pp. 226. Price 10s. 6d. **25**

This volume is an introduction to the study of social organization. Dr. Rivers had in contemplation the revision and enlargement of the lectures, embodied in the present work, for eventual publication: this was frustrated by his untimely death. The unrevised lectures, twelve in all, were, however, admirably suited for publication in the form of a small book, introducing the student to a difficult subject with Rivers' characteristic lucidity.

Although the manuscript of the lectures was fairly complete, an editor was faced with certain difficulties, owing to doubtful passages and blank spaces in the manuscript. Mr. Perry, who undertook the task of editing the volume, decided, on the advice of Professor Elliot Smith, to recast somewhat drastically the lectures dealing with sibship, kinship, and marriage. The work has gained by a rather fuller account of these features of social organisation than appeared in the original manuscript. This expansion and modification of the original lectures has been carried out in part by embodying relevant extracts from Rivers' earlier works "Kinship and Social Organisation" and "History of Melanesian Society." Although the rewritten chapters no doubt express, on the whole, the point of view

held by Rivers at the time of his death, the rewriting seems to have led to a slight change of emphasis. The insistence on exogamy as the chief characteristic of the clan is not apparent in Ch. II as rewritten, nor is it clear in this chapter that all that Rivers meant by a "Dual Organisation" was a division of the tribe into two exogamous moieties. The footnote on p. 31 is, therefore, either misleading or superfluous. Attention should be drawn to an important oversight on p. 28, line 15: for "the two moieties" read "different clans." In the chapter on "Marriage," a statement is made for which there seems to be no authority in Rivers' manuscript, namely, that in the regulation of marriage by clan or moiety "it is always found that kinship plays some part, in that certain relatives are favoured as regular mates." The first part of this statement, the only part which occurs in the original manuscript, implies no more than that certain relatives, between whom marriage is not prohibited by the clan or moiety organisation, are debarred from marriage by the family organisation. Even though Rivers may have accepted A. R. Brown's theory of the regulation of marriage in Australia by kinship-injunction, there is no reason to suppose that he believed this to be the case wherever we have the clan or the moiety. It is interesting to note that in the same chapter the Eddystone Island system of marriage is recorded, in which a person is prohibited from marrying anyone who is related by kinship. Ch. IV., which has been expanded and rearranged by Mr. Perry, gives an excellent account of kinship and the classificatory system, but it must be remembered that "blood relatives" is not a term used by Rivers and is unfortunate, suggesting, as it does, consanguinity, where kinship only is meant. The reference on p. 68 to "Melanesian Society" should be corrected; evidently p. 16 is intended, not pp. 77-8. Mr. Perry has added a useful section on "relatives by marriage" to show how the possible marriages in a "Dual Organisation" lead to the various forms of classification of kindred with relatives-in-law, which we find in the classificatory system and which is discussed in "Kinship and Social Organisation." It is unfortunate that on p. 74 the list of kindred and close relations-in-law, who are possible mates for a man, given a dual organisation and matrilineal descent, is incorrect and incomplete. A man clearly could not marry his father's brother's wife and there are many relatives whom a man could marry other than those given.

It may be that there is an intimate connection between the classificatory system and the dual organisation, but Dr. Rivers does not commit himself to this point of view, as is made clear in the essay, reprinted in Appendix I. The theory of group-marriage is put forward in this essay to account for the extension of relationship-terms to persons with whom genealogical connection cannot be traced. This theory is independent of the fusion-theory, which Rivers put forward in "Melanesian Society" to account for the "Dual Organisation" and the classing of kin with relatives by marriage, a classification which logically is possible without there being a classificatory system of relationships in the original meaning of the term. The failure clearly to differentiate these two senses in which a system of relationships may be classificatory has confused the problem. Whereas Rivers has given an adequate explanation of the classification of kin with relatives by marriage, the theory of group-marriage is his only attempt at explaining the extension of relationship-terms to all the members of a clan or moiety; he was, moreover, aware of the difficulties of this theory. Mr. Perry, in Appendix III, offers a totally different theory of the origin of the "Dual Organisation" from that of Dr. Rivers. The "Dual Organisation," on Mr. Perry's theory, has been impressed on the aborigines of various parts of the world by immigrants, few in numbers, who brought with them an organisation into two divisions, having matrilineal descent with a bias in favour of marriage with kin of the opposite division, mitigated by occasional incest. If this incest-tendency be disregarded and this bias be taken as a definite injunction to marry the cross-cousin or the

brother's daughter's daughter or some other appropriate relative, and if it be assumed that one or other of these forms survived and became universal for a time in various parts of the world, then we have an explanation of one feature of the classificatory system and of the apparent exogamy of the moieties. There is an inherent improbability in this theory even for one who accepts the main contentions of the "Diffusionist" school. Moreover, it must be borne in mind that it offers no explanation of the main feature of the classificatory system, the use of relationship-terms beyond the realm of genealogical memory.

To return to Dr. Rivers' treatment of social organisation and particularly his classification of social groups, some confusion seems to have been made by the failure to distinguish social groups which are absolute, such as the clan, from groups which are relative, such as the family, when defined bilaterally by means of the concept of kinship. Rivers, for instance, gives as an example of the bilateral family a group called "taviti," the "most important social group on "Eddystone Island." It consists of "all those persons with whom genealogical "relationship other than by marriage can be traced, whether through the father "or the mother," and it is suggested that this is an exogamous group, even though it contains a person's father and mother. This so-called group of kindred, the "taviti," cannot be a social group any more than there can be a social group of uncles. "Taviti" is clearly only a relationship-term of wide classification.

Little need be said of the chapters dealing with other features of social organisation, the treatment of which, though brief, is clear. The comparison of Melanesian with African Secret Societies is of special interest, and also the discussion of the communal mechanism of government which sometimes occurs in association with chieftainship.

W. E. ARMSTRONG.

Canada : Ethnography.

Barbeau.

Indian Days in the Canadian Rockies. By Marius Barbeau. Illustrations by W. Langdon Kihn. Toronto : The Macmillan Company of Canada, 1923. 26 Pp. 208.

"Indian Days": this was the name found for certain historical pageants held near Lake Windermere in 1922, to celebrate the first coming of the white explorers and traders to that country. The Indians of the neighbouring reservation joined in the show; painted their faces, put on their old-time finery, and rode in parades reminiscent of the buffalo hunt and of native warfare. From these "Indian Days"—"Dias de los Muertos" they might well be called—Mr. Barbeau turns back to "the real Indian days that are no more," to "the time when the white man, long predicted by native seers, was first seen by the north-western tribes," Crees, Blackfeet, Stonies, Piegans, Kootenays

In this deeply interesting and unusual book (made more arresting by Mr. Kihn's beautiful portrait drawings, with an air of patience and fate about them) there is small comfort for lovers of the Indians. "It is clear," says Mr. Barbeau, "that "the Indian, with his inability to preserve his own culture or to assimilate ours, is "bound to disappear as a race. It is one of the great tragedies of the American "continent." He approaches this tragedy by showing us various aspects of the white man's coming to the north-west—fire-arms, and the disastrous wars that followed on their possession, gaining no permanent advantage for any tribe, but rather clearing the country for the invader—disease—the unequal trade of necessities for luxuries—the old story of Caliban and Trinculo. There is the curious life-story of Tchatka, the cunning unscrupulous man who made the new knowledge serve his ambition. Most curious of all, the story of the rumours which ran before the white man, of the native prophets who preached his coming as a new religion, promising a golden age, and of their disillusionment.

"Conservative and unprogressive as the north-western tribes may seem to us at the present day, it is nevertheless true that they have failed to safeguard the most fundamental features of their traditional culture. Hardly any of their manual arts have resisted the impact of trade wares and goods. though some of them were truly fit to survive for the benefit of the community at large. The riches of their mythology have unnecessarily fallen into discredit, to be replaced by inept distortions of Christianity or mere vacuous scepticism. The premature disappearance of many ancient customs that lay at the root of native ethics has brought the whole social fabric to the brink of the abyss. Self-confidence is gone beyond redemption; pride and ambition ruined; and hope in the future is slight, if not futile.

"Before the white man was first encountered, he was looked for as a demi-god. His coming was greeted as a great blessing. The golden age seemed to be dawning. The heirlooms of the past were cast aside; their discredit was absolute, final. But the day of awakening was not long in coming. The white invaders were not 'Sky-beings,' they were not the benefactors of any but themselves. The possession of their goods, their riches, was not enough in itself to make life more happy. Prosperity as a result did not fall to the lot of the red men, the first occupants of the land: for after more than a century of faithful service to the fur-trading companies, they have not managed to secure themselves against the growing prospect of a rainy day. Nor was their improvidence wholly to blame, for no European, however thrifty, could have stored away the perishable and scanty proceeds of the trade in the trying circumstances of nomadic life" (p. 80).

B. A.

CORRESPONDENCE.

Psycho-Analysis.

Jones, Wohlgemuth.

*To the Editor of MAN.***Psycho-Analysis or Anthropology.**

SIR,—In reply to Mr. Hocart's criticism (MAN, 1925, 6) I can only plead: **27** *mea culpa*. He is, of course, right when he points out that our "cleave" is a modern fusion of what were two different words. (Whether, as seems probable on linguistic grounds, these two words themselves emanated from a common root cannot be demonstrated in the present state of our knowledge and so I will not defend the example.)

But while my hurriedly choosing a bad example justly lays me open to being "scored off," it would be evidence of very considerable bias to assume that the general proposition itself can be settled in any such off-hand manner. This is to the effect that the further back we trace language, as every philologist who has investigated the matter will agree, the more closely associated do we find "opposites" to be, and the point I was making in my paper was that this interesting observation is also made when the various layers of the mind are investigated comparatively, for instance, by the study of dreams. It is not the place here to quote evidence in support of this correlation; part of it is given in Freud's "Collected Papers," Vol. IV, No. 10.

Further, in regard to the inference Mr. Hocart draws, I am glad to be able to assure him that, like several other ethnologists, he is quite under a misapprehension in supposing there to be any incompatibility or conflict between the historical and the psycho-analytic methods of research. On the contrary, the bulk of our work, notably in tracing the detailed genesis of psycho-neurotic symptoms, is essentially historical, and the main thesis of psycho-analysis is that the meaning and significance of the present is determined by its historical past in much greater minuteness

than is generally appreciated. Our psychology, therefore, rests entirely on history. It being so evident that the historical and the psychological approaches to anthropology are mutually dependent and complementary, it would be unfortunate if investigators exploring along these lines should experience any difficulty in co-operating to achieve their common goal, viz., fuller knowledge and understanding of man.

Yours, etc.,

81, Harley Street. W. 1.

ERNEST JONES.

To the Editor of MAN.

SIR.—I have read with much interest in your January issue the letter of your correspondent A. M. Hocart and shall be pleased if you will allow me, as a psychologist, to say a few words in reply.

First.—I should like to point out to your correspondent that he confounds *psychology* and *psycho-analysis*. Psychology has become an exact science and as such relies upon experiment as much as Physics or Chemistry: its method is quite as rigorous. The strictures of your correspondent apply rightly to psycho-analysts who have dubbed their doctrines the "New Psychology." The experimental psychologist, as much as the historian, rejects the psycho-analytic work and finally refuses even to read it.

Secondly.—From the foregoing remarks you will see that I am heartily in agreement with your correspondent respecting this pseudo-science and should like to supplement his remarks by the following instance. Dr. Ernest Jones states in his "Papers on Psycho-analysis," London, 1918, p. 142-3: "Even with symbol " words, where it is hard to trace any association between them and the words " denoting the ideas symbolised, such an association is often apparent in the case " of synonyms of foreign equivalents. A good example is our word 'room'—a " room is a regular unconscious symbol for woman—where one has to go to very " remote Aryan sources—*e.g.*, Old Irish—to find any trace of feminine connotation; " one has only to turn, however, to the German equivalent, *Zimmer*, to find that " the compound *Frauenzimmer* is a common colloquialism for woman." What the evidences of "Old Irish" are I should like to know, but we are not vouched this information. As to the German word *Frauenzimmer*, it is evident that we have to do here merely with a trope, a metonymy. The "*Frauenzimmer*" was the "Women's Room," corresponding to the Greek "*Gynaikonitis*," where the women of the mediæval castle congregated, worked, or were entertained. Not before the sixteenth century was the word used to apply to the occupiers of the *Frauenzimmer* and at first only collectively, as we still to-day speak, *e.g.*, of a school, meaning scholars, the school was present at the cricket-match; or, all London was at Wembley, *i.e.*, all Londoners were at Wembley, and so on. According to Grimm, *Frauenzimmer* was at first the room in a Knight's Castle where the women congregated; then the inhabitants of that room collectively; next, women in general; next, a woman of good birth and breeding; since the latter half of the eighteenth century, any woman; and to-day it is mostly used in a derogatory sense. The Dutch equivalent *vrouwentimmer* has retained its original spatial meaning and has not been extended like the German word.

Yours truly,

A. WOHLGEMUTH.

Through the courtesy of the Editor I have the opportunity of commenting on Dr. Wohlgemuth's letter. (1) Mr. Wohlgemuth is wrong in implying that "the experimental psychologist rejects psycho-analytic work," the truth being that some experimental psychologists have investigated and accepted it, while others reject it without investigating it. Further, it would be hard for him to find any psycho-analysts "who have dubbed their doctrines the New

Psychology," though the expression is often used of psycho-analysis by non-analysts. (2) Mr. Wohlgemuth's comments on my note on the word *Frauenzimmer* might have been more helpful if he had indicated the relevancy of them. The facts he brings forward were, of course, well known to me when I wrote my note, and I might have included them in the same context as a direct amplification of it. They illustrate very well my contention that the identification which appears in one place as unconscious symbolism can appear in another place as simple metonymy or other form of metaphorical expression. If Mr. Wohlgemuth considers that his remarks in any way contradict my argument, then he must certainly have misunderstood my argument.

ERNEST JONES.

Britain : Archæology.

Dawkins.

*To the Editor of MAN.***Late Palæolithic Art in the Cresswell Caves.**

SIR,—I write as Chairman of the British Association Committee, now **28** resuming, by permission of the Duke of Portland, the exploration of the Cresswell Caves, where it was dropped some 40 years ago by the Reverend Magens Mello and myself, to prevent your readers from being misled by the following passage in the last edition of Professor Sollas's book on "Ancient Hunters." p. 536 :—"There is a singular absence of any attempt at art in all the paleolithic " Stations of England. The horse figured here (Fig. 299) is, I am assured, a " forgery introduced into the cave by a mischievous person."

The Cresswell horse was the first proof of the range into Britain of the wonderful art of the French Caves, and the discovery made in the seventies by myself was published, after a careful scrutiny by Sir John Evans, Sir Augustus Franks, Lord Avebury, General Pitt-Rivers and other leaders, in the quarterly Journal of the Geological Society of London. It has remained unchallenged for more than 40 years, and has passed into the literature of anthropology. *Res judicata est.* The charge of forgery is not now to be made without clear evidence. In answer to a letter asking for this, Professor Sollas writes to me that it is based on what he was told "some years ago, I think 1919" by a clergyman since dead, who declined to give names or other particulars. This means that the charge of forgery is founded on gossip without a shred of evidence and unworthy of further notice.

The Cresswell horse is engraved in fine lines in a style similar to that of the figures of animals found since in the late paleolithic caves of France and Switzerland. It is not accurately represented by Professor Sollas's Fig. 299. This is copied from Evans's "Ancient Stone Implements" (2nd Edition, p. 524), in which my woodcuts were used. If the copy be compared with the original it will be seen that the details have been omitted, leaving merely an outline useless for the study of the art of the Caves.

Professor Sollas is equally unfortunate in his sweeping statement that there is no attempt at art in the paleolithic caves of England. Our Committee is now at work at Cresswell, and Messrs. Garfitt and Leslie Armstrong have already recorded the discovery of incised figures of bison and reindeer along with other late paleolithic finds. As the work proceeds it will probably result in further proof that the picturesque gorge of Cresswell Crags was a hunting station of the artistic tribes who followed the wild animals in their migrations from the South of France into Britain, then the north-western region of the great Pleistocene Continent.

Yours, etc.,
W. BOYD DAWKINS.



STATUE-MENHIR FROM TRAMIN, SOUTH TYROL.

ORIGINAL ARTICLES.

With Plate D.

Tyrol: Archaeology.

Menghin.

A Statue-Menhir from Tramin, South Tyrol. By Professor
Oswald Menghin, Vienna. With Plate D.

29

In the Ferdinandeum, the provincial museum of the Austrian Tyrol at Innsbruck, an interesting sculptured stone has lain unrecognised for many years. Although it has been seen by many archaeologists, it has never been described, perhaps because no one liked to take the risk of assigning a date to a prehistoric monument which is unique in this region. It was found in a field of the hamlet of Rungg, in the commune of Tramin, and was taken to the Museum at Innsbruck during the 19th century. Unfortunately, nothing more is known about the circumstances of its discovery.

The material is sandstone; its height is 181.5 cm., breadth 57.65 cm., thickness 25.77. As the illustration (Plate D.) shows, the monument is shaped like a column with square section and with a triangular top. All the edges have been intentionally rounded off, and the surface is everywhere worked. There can be no doubt that the marks are intended to represent a human figure. Unfortunately the stone is fastened so securely to the wall that it is impossible to see anything on the back. But the front and sides show clearly that an armed man is represented. On the top is seen a row of small parallel vertical strokes, suggesting a necklace. The face, however, is not indicated. Below this row, on the "man's" left, is the engraving of a dagger, of which the blade is quite clearly visible, while the handle cannot be detected. Below this is another smaller dagger, and two identical engravings on the right side of the stone. The first and bigger dagger is in a vertical position; the three smaller ones are horizontal. It is important to note that the smaller daggers are executed in a different style from the other, which is shown in strong outline, whereas they are shown by chiselling away the whole surface. The pommel and blade are represented by a hollow, the handle by a rib, left in relief by an artificial cavity on each side. It is not easy to discern these three daggers on the monument itself, but they can be clearly seen in the photograph. The marks below the necklace are certainly of quite modern date. These too can be seen better in the photograph than on the original; the figures of a year and two capitals, probably 1881 A.D., can be read, possibly the date of the discovery. Round the middle of the figure runs a broad belt consisting of eight curved lines. On the two narrow sides can be seen many oblique ribs, which may be intended rather for the folds of a garment than for those of a human body; for they occur not only on that part which may be regarded as the breast but also on what is probably the shoulder. The lower half of the statue shows no trace of engravings.

The question of date is a very difficult one. Every archaeologist will be reminded at once of the celebrated statue-menhirs or dolmen-ids of Southern France and Upper Italy. And, indeed, there is a close connection between the Tyrolese monument and these others, particularly the French examples. Many of the French statues, such, for example, as those of Saint-Sernin and Montel (Aveyron), have the necklace and belt. Some have only the belt or only the necklace. In the best French examples the face, arms and feet are shown, but they are often absent; and feet and arms are never shown otherwise than in outline or low relief. The method of representing the belt on the Tyrolese statue is peculiar. On the French statues, curved lines do not appear, only straight, moulded lines, irregularly ornamented by oblique strokes (Les Maurels). On the female statue of Saint-Sernin the dress is indicated by long vertical ribs, suggesting a wide gown, also covering the shoulders. The points of connection with the statues of Fivizzano,

near Spezia, are much less numerous, in spite of the geographical proximity. But there is one important link between ours and the Italian group—the daggers, as represented on the Fivizzano stones, are of the same type as the three small daggers on the Tyrolese statue. Both sets have triangular blades, narrow handles and semicircular pommels. The engraved daggers are not entirely absent on the French statues, but they occur only very rarely and belong to a different type. The big dagger on the Tyrolese statue has a certain similarity to the dagger represented in the wall-painting of Peña-Tu in Northern Spain; and this type is usually attributed to the earliest Bronze Age. We may place the Fivizzano monuments in the same period, and consequently the Tyrolese statue from Tramin may also belong to the same age. Typologically it seems to represent an intermediate or transitional stage between the French and Italian types of statue-menhir. Neither the Italian statues, nor those French ones in the Department of Gard possess any sculpturing on the back of the stones. Perhaps the same is true of the Tyrolese example. All these elements suggest, according to our opinion, that the statue of Tramin has to be connected with the statue-menhirs of the West European culture-circle. Such a connection would not be surprising, for, as I have shown in dealing with the Tyrolese stone age,* the neolithic culture of the Southern Tyrol is in every respect closely connected with that of Italy.

The late M. Déchelette, and others of his school, regarded the statue-menhir as representing a god or goddess of death. But the very fact that we can distinguish male and female images is a point in favour of another theory, according to which these sculptures are images of dead persons buried originally at the feet of the monument itself.†

OSWALD MENGHIN.

Obituary.

Myres.

Ernest Chantre. *By Prof. J. L. Myres, M.A., D.Sc., F.B.A.*

30

The death of M. Ernest Chantre on 24th November of last year, at the age of nearly eighty-two, closes a career of incessant and valued activity in the study of prehistoric archaeology. Born on 13th January, 1843, and the author in 1867 of an important study of the Stone Age in the neighbourhood of Lyons, Chantre joined the staff of the museum of that city in 1871, and remained in its service till his retirement as honorary director in 1910. In addition to his duties as curator, he lectured regularly in the museum on geology and anthropology, in the Faculty of Sciences of the University of Lyons on anthropology from 1881 to 1908, and in the Faculty of Letters on ethnology from 1892; and in 1903 he was invited by the city also to lecture on anthropology to students in the "Enseignement supérieur." The Société d'Anthropologie de Lyon honours him as its founder and general secretary. Here was a programme which would have made a busy life for many able men: but Chantre was, in addition, a voluminous writer on prehistoric and general archaeology, an authority of the first order on the physical and human geography of the Rhone basin, an active member of innumerable archaeological commissions, congresses, and other learned bodies: and he received the quite unusual distinction, for a specialist in the historical sciences, of promotion to the rank of Officer of the Legion of Honour. Lavish of his time and knowledge, to colleagues, students, museum visitors, and a wide acquaintance in his own city, he is probably best known abroad for his travels in the Near East, and for the magnificently illustrated monographs which record the results of these journeys: five great volumes on the Caucasus; two on Tripoli, Tunis, and Algeria; one each on Transcaucasia, Cappadocia, and Egypt with Nubia. In these "missions anthropologiques" he

* *Jahrbuch für Altertumskunde* (Vienna), VI., 1912, p. 91.

† For fuller discussion of this theory see Hoernes-Menghin, "Urgeschichte der Kunst," 3rd edition, 1925, p. 686.

was most ably assisted by his wife, who survives him. He had seen prehistoric archaeology become a branch of organised science through the excavation of stratified deposits in the Swiss lakes, and had himself contributed one of the earliest monographs on similar sites in France—"Les palafittes ou constructions lacustres du lac de "Paladru, près de Voiron" (Chambéry-Grenoble, 1871); he succeeded, where so many have failed, in at all events probing the archaeological mysteries of the Turkish Empire; he was a pioneer in more than one other region, where others have entered fruitfully into his labours; and his wide knowledge, sound judgment, and capacity for persistent, unassuming, efficient service in the cause to which his whole life was devoted, have assured his place among the master craftsmen of modern anthropology.

J. L. MYRES.

Africa : Sociology.

Seligman.

Marital Gerontocracy in Africa. *By Brenda Z. Seligman.*

I received the following information from Mr. C. K. Meek too late for inclusion in my article on this subject in the recently issued Vol. LIV, Pt. 2, of the *J.R.A.I.* :—

31

The Katab tribe in Zaria, province of Northern Nigeria, use similar terms for "grandfather" and "grandson" to those used by the Gbari; they call each other "wife-stealer." A man's son's son (but not his daughter's son) may inherit his grandfather's widows if they are of a suitable age. The Muslim Nupe as well as the Muslim Hausa both make fun when a grandson enters his hut, saying: "Well, what do you want? Have you come to steal my wife? Get out!"

This additional information shows how firmly established the marriage with the grandmother must have been in Northern Nigeria.

BRENDA Z. SELIGMAN.

Ethnology.

Haddon.

Pearls as "Givers of Life." *By Dr. A. C. Haddon, F.R.S.*

32

Mr. Perry seems to assume that I am attacking the general theory associated with his name and that of Professor Elliot Smith.* This is by no means the case, as I agree that higher cultures have entered into the Pacific and have been subject to modification and degeneration, but as a preliminary to accepting any theory it is necessary to make sure of the data upon which it is founded and by which it is supported. Mr. Perry is also a little unkind in presuming that I pay no attention to the possibility of degeneration and that I desire to demonstrate advance in all places; it should be needless for me to assert that the reverse is the case.

The linguistic points must be left, as Mr. Perry says, "for discussion between the experts." He refers to my "unkind treatment of the distinguished scholars" who helped me, and adds that I quote "the opinions of both of them presumably without letting either see his colleague's statement. . . . Had Dr. Haddon been content with the opinion of one Professor of Arabic, a serious difficulty might have had to be met." As a matter of fact, each of my friends saw before publication what the other had written. My object was to get as accurate information as possible about the word *marjan*, and I was not attempting to support or combat any particular theory. If by "naively" Mr. Perry means "honestly," I accept the jibe, as I have no intention of suppressing evidence; this is shown by my quotation from Professor Rapson, and I would like here to refer the reader who is interested in the subject to the *Journal of the Bihar and Orissa Research Society*, X, 1924, pp. 194-5, 250, 252.

I am afraid that I have overlooked the reference in Dr. Fox's book, "where it is definitely asserted that pearl-shell is a 'giver of life' in San Cristoval."

* See MAN, 1925, 22, 23.

Dr. Fox says that "there are certain objects which are *par excellence* the vehicle of *mena*," such as mother-of-pearl, certain shells, etc. "These things are, as it were, good conductors of *mena*, and, once saturated with it, they will not quickly or easily lose it" (p. 253). "Most instruments are charmed, such as the pearl-shell for slicing yams (and then yams sliced with this and planted will bear well)" (p. 260), which looks as if there was not sufficient inherent virtue in the shell itself.

Mr. Perry claims "that the San Cristoval attitude towards the pearl-shell is the original attitude, and that all such ornaments were used in the first instance as amulets." It does not, however, necessarily follow that the material of which an amulet is made has in itself any special property—for example, wood or turtle-shell. Every ethnologist knows, as Mr. Perry says, that "amulets of pearl-shell are made in other islands of the Solomons," and he refers the reader to Ribbe; the unwary might very well believe that all the references given to Ribbe related to pearl-shell "amulets," but, as a matter of fact, three of the five objects referred to are made of "Conus or Tridacna."

Mr. Perry naturally lays stress on the fact, pointed out by Chinnery, that megalithic monuments and irrigation occur in the neighbourhood of goldfields and pearl-beds in New Guinea. On p. 98* of "The Children of the Sun" this curious statement is made: "The implement-makers of New Guinea, the gold-hunters of the mountains, are gone, and the existing natives neither make nor use such implements." It ought to have been superfluous for me to state that not only I, but other travellers, have described and figured the use of stone implements at the present time by natives of New Guinea. Neuhauss refers to the very hard serpentine which is found in scattered blocks on the Kela peninsula (Samoa Harbour, Huon Gulf), from which were made, up to the present time, the stone axe-heads of that neighbourhood ("Deutsch Neu-Guinea," I, 1911, p. 140). He also says that while places are known where up to the present day stone axes are being made, it is quite impossible to determine the original source of the stone clubs (*l.c.*, p. 145). Mr. Perry seems to imply that all the stone implements were made by a vanished people, which appears incredible when one realises the very great number of such implements in collections, and their abundance still in New Guinea.

In the present state of our knowledge it seems desirable to distinguish between those areas (i) in which pestles and mortars, but no megalithic structures, have been discovered, and (ii) those in which megalithic erections are known, but where no pestles and mortars have been found.

(i) The first area includes the south of the Huon Peninsula and the northern part of the south-eastern peninsula from the Waria to the Lakekamu on the north, extending as far south as a line drawn from Gona on the east coast to Port Moresby on the west coast. The finds are associated with modern gold-fields and the assumption is that the pestles and mortars were connected with gold-seeking operations of a previous population. (Chinnery, *J.R.A.I.*, XLIX, 1919, p. 271). Somewhat farther south two pestles were found at Cape Nelson by Captain F. R. Barton (MAN. VIII, 1908, p. 1); but this district does not appear to be a gold-field, and another pestle, referred to by Chinnery, was found in Murua (Woodlark Island) a well-known gold-field. Some of these pestles very closely resemble some of the taro-pounders of Polynesia. Neuhauss says that, although the Papuans do not use them for this purpose, it is evident that nevertheless there is a dim idea that they are in some way associated with taro (*l.c.*, I, p. 140).

* On the same page we have the astonishing statement that some of the Australians speak an Austronesian language, and this is utilised as an argument for the influence of the archaic civilisation in Australia.

(ii) The second area includes Goodenough, Bartle and Milne Bays, the D'Entrecasteaux, etc. Seligman says ('The Melanesians of British New Guinea,' 1910, p. 463): "At Rogea, and apparently on very many islands of the south-eastern district, the majority of the hamlets have in or near them a collection of large stones, or a heaped mass of smaller stones, used as the squatting and yarning place of the men." There is no evidence to connect spots on the mainland where stone-circles occur with the modern Gibara gold-field. The Massim culture is so very different from that of the first area that each must have had a different history, and should the foundations of their respective cultures be proved to have had ultimately the same source, the divergence between them requires explanation. Terrace cultivation occurs at places in the two areas, though apparently very rarely in the first, and certainly irrigation is practised in the second area. One should be careful to distinguish between irrigation and drainage.

From this discussion we have learned that when Mr. Perry wrote "pearls" he did not necessarily mean pearls. We may put down as printer's errors the omission of the "r" in *magarites* and the misspelling of *kemerkemer*, but *op. cit.*, in alluding to the Torres Straits reports, refers to Vol. VI and not to Vol. IV (MAN, 1925, pp. 39, 41).

It is not at present my intention to enter into either a further discussion or a "full discussion" of Mr. Perry's theories: indeed, it would almost require a syndicate to check his references and statements and to collect other evidence, for this would have to be a necessary prelude to satisfactory discussion. All writers are liable to slips, but careful students always bear in mind that the historical, as well as the scientific, method demands that, among other matters, one should say exactly what one means, make as certain as possible of the data, quote correctly, consult the original sources at first-hand, and that a clear distinction should be made between assumptions and definitely ascertained facts.

A. C. HADDON.

Sociology.

Seligman.

Problems of Social Organisation. By Brenda Z. Seligman.

33

It might well be supposed that *Social Organisation*, by reason of its comprehensive title and its appearance as the final production in the series of posthumous works from Rivers's pen, should be regarded as an epitome of his views on the subject. Unfortunately, Rivers did not prepare his material for publication and this volume can in no way be looked upon as a complete treatise on Social Organisation.

It is inevitable that there should be certain inconsistencies and contradictions in a work of this complexity; but, rather than blame the editor for this, these very blemishes must be taken as evidence of the faithfulness with which he has performed his difficult task. Nor has Mr. Perry failed to give full prominence either to evidence or to expressions of opinion which may be regarded as being in direct opposition to his own theory of Dual Organisation, which is printed as an appendix to this book.

In the preface to *Medicine, Magic and Religion*, Professor Elliot Smith hinted that Rivers was becoming "emancipated" from the view that dominated his thoughts between 1911 and 1918—that is to say, the importance of the blending of different cultures. Since this view has been extraordinarily stimulating and fruitful, and *The History of Melanesian Society* had been written under its dominance, all students will scan *Social Organisation* with the keenest interest, appreciating the importance of any changed outlook, and it is in order to consider this matter that the following article is written.

At the outset it may be stated that the work under consideration shows no fundamental change in point of view. Those who have admired Rivers's work and been influenced by it will welcome the statement on p. 97 "that the existing

" institutions of mankind are not the result of a simple process of evolution, but " that there has been in action a highly complicated process of blending and " interaction of cultures, often widely different from one another, the outcome of " the interaction being complex structures, not only containing elements derived " from both the blended cultures, but also new products of the interaction."

As an appendix to this book a paper on " The Origin of the Classificatory System " of Relationships," published in 1907 in *Anthropological Essays presented to Edward Burnett Tylor*, has been reproduced. In *Kinship and Social Organisation*, published in 1914, Rivers discarded the term " group marriage " used in that paper in favour of " organised sexual communism."

In the earlier paper republished in this book (p. 187), Rivers wrote:—

" I shall have to begin by making certain assumptions. First, I assume that* at " the time the classificatory system had its origin, *the custom of exogamy was already* " *in existence*, and, further, I assume, for the sake of simplicity, *though it is not* " *essential to my argument, that the community possessed only two exogamous sections,* " *which I will call moieties*. We now have so much evidence of such a dual division " of early society that there are few who will object to this assumption, *though my* " *argument would apply equally well if there were more than two exogamous divisions* " *of the community*.

" Further, I assume, again for convenience sake, that the *child belongs to the* " *division or moiety of its mother*."† (P. 187).

Throughout *Social Organisation* the first assumption is still unquestioned, but the possibility of the existence of more than one group is rejected.† The third assumption must be considered more carefully. In the first few chapters, which deal with the complexities of various social groupings, including the clan and the origin of the classificatory system, it remains unchallenged. Chapter V is devoted to father-right and mother-right; after rejecting the view that mother-right must everywhere precede father-right, the opposite hypothesis is considered to be less tenable. It is stated (p. 99) that in a food-gathering stage men must have lived in " small loosely defined bands, the social processes we call descent, inheritance and " succession would be of a vague indefinite kind, and might, in many cases, hardly " be said to exist at all. If the consequent growth of the groups in size led to the " formation of clans, it becomes possible that the evolution may have taken place in " two directions, producing patrilineal and matrilineal institutions respectively. " In some cases the loose band may have evolved into a patrilineal clan without any " intermediate stage of mother-right The situation is one for an open " mind."

How are we to reconcile this statement with the theory given in this book on the origin of the classificatory system? For the classificatory system itself is dependent on the clan.

Chapter IV deals with kinship and relationship systems. It will perhaps be well to quote a brief account of the classificatory system that Rivers published in 1912:—

" The simplest way to understand its essential characters is to recognise that " the classificatory system is founded on the clan or other similar social group, " while our system is founded on the family. All members of the speaker's clan " who are of the same generation as himself, stand to him in the same relation as his " own brothers and sisters; all members of his father's clan of the preceding " generation stand in the same relation to himself as the father or the father's sister; " all of the generation before receive the same designation as his grandfather.

* The italics are mine.

† I refer only to the part of this book under Rivers's name—not to Mr. Perry's appendix.

" Similarly, all those of the mother's clan and of her generation are classed with the mother and the mother's brother; all those of a wife's clan and of her generation stand in the same relation as her actual brothers and sisters, while all of the preceding generation are classed with her parents."*

The matter is not treated in precisely the same way as in this book. Here we are told that this system "accompanies" moieties and clans, that if we only had to explain the distinction made between blood relatives, the origin might be sought in the dual organisation or the clan system. "But certain distinctions between relatives suggest strongly that the dual organisation was the sole source of this system of relationship and that, consequently, the clan grouping only enters incidentally into the matter." (P. 67). Frankly, I do not understand this sentence; one can have a clan system without dual organisation, but is the reverse possible? It can scarcely be supposed that Rivers intended to discard all that he had previously written on the close correlation of the clan and the classificatory system without weighty argument. That a clan grouping could be put on one side as "incidental" (whatever that may mean) is not in keeping with his previous work.

So far as blood relations are concerned, and only two groups are assumed (according to the hypothesis in the main part of his book, but contrary to that in Appendix 1), it would make no difference whether the descent were matrilineal or patrilineal. But in order to account for relatives by marriage being addressed by terms otherwise used to blood relations, certain marriages are suggested to have been habitual, all of which occur in Melanesia. A diagram is given on page 74 showing which women can be married in a society with dual organisation and matrilineal descent. Of these the cross cousin marriage, the marriage with the father's brother's wife, the mother's brother's daughter and the brother's daughter's daughter, would be allowed whether descent were in the male or female line. No suggestion is made in this chapter that descent might be in the male line, but in view of the opinions expressed in Chapter V, it becomes necessary to test the validity of this hypothesis with descent in either line. If, therefore, a similar diagram is made with patrilineal descent, it will be seen that three other marriages, which are necessary to this hypothesis, would not be allowed, namely, with the father's sister, the mother's brother's wife and the brother's daughter, while yet three other marriages would become possible which are actually not allowed in Melanesia (and must be extremely rare if permitted at all among ruder cultures). These are with the mother's sister, the mother's sister's daughter, and the sister's daughter.

In other words, dual organisation divides the women of a society into two groups, marriageable and non-marriageable, and it is only on the assumption that descent was matrilineal when the dual organisation was formed, that Rivers's theory of the origin of the classificatory system will hold good. If at this time descent had been patrilineal, though a certain number of marriages would have been the same, the marriageable women possible would have formed a different group; three other marriages would now be included, and we should have expected to find them as common as those three which, with male descent, are excluded: whereas, so far as I know, the first two are non-existent and the third is not at all common.

If, then, we accept Rivers's theory of the classificatory system, what are we to think of the priority of matrilineal or patrilineal clans?

It is obvious that the third assumption that Rivers made in his original thesis must be retained if the theory is to remain, and that, as far as clan grouping goes, female descent must be an older organisation than male descent. This, of course, has no bearing on the earliest human family group, where descent and succession

* "Notes and Queries on Anthropology," . . . fourth edition . . . 1912 . . . pp. 149-150.

would have been unimportant, but authority would, doubtless, have been wielded by the father. I do not believe that Rivers meant to suggest that the clan system evolved from a loosely defined band and developed into a fully organised clan before the classificatory system came into being at all. I have supposed that, with the improvement in methods of procuring food which made it possible for larger groups than a family to live together, the necessity arose for ensuring the solidarity of the group by preventing internal disruptions, and that this stimulus caused the clan organisation and the classificatory system to develop contemporaneously.

It is clear from Mr. Perry's appendix that his own view is quite different; there is no evidence to show that Rivers had adopted this view of sociology which leaves on one side the most important part of his own field of work in social organisation, as well as his written work on the gregarious instinct. This point must be referred to again when considering chapters VI and IX. but it must be noticed that, if the Editor's hypothesis of dual organisation be accepted, the Rivers theory of the classificatory system must be rejected.

The extent of Rivers's adherence to the historical school of anthropology, however, is another matter, and may be seen best in Chapter VII, which is devoted to the Fraternities and Secret Societies. In this chapter he ascribes the similarities and differences in such societies, throughout the world, to the *interaction of one immigrant culture* on different indigenous peoples.

In the present book Rivers pays more attention to the importance of the family than in his previous works; those who have criticised him formerly should note that he specifically states that the use of classificatory terms of relationship does not imply any lack of recognition of the true parents. Moreover, that when classificatory terms are used all persons classed together are not of the same social importance, the family is considered to have a social function, viz. "to assign to each individual 'born into a society the special place which he or she is to occupy in that society.'" (P. 6).

A slight modification is seen in the treatment of the relationship systems from that adopted in *Kinship and Social Organisation*. In that work he divided the systems into three kinds—the family system (our own), the kindred or extended family, and the classificatory system which is characteristic of clan organisation. He now separates the "Kindred" group from the "Joint Family" group; the "Kindred" group is defined as the bi-lateral joint family, and the term "Joint Family" is reserved for a unilateral group, which, again, is subdivided as matrilineal and patrilineal.

When nomenclature is considered, this grouping is difficult to follow; our nomenclature is associated with the family, and the classificatory system with the clan. What, then, is the influence of these two forms of social grouping on nomenclature? Rivers had apparently not revised his previous consideration on this matter, and the "Kindred" system is still associated with that kind of nomenclature found among the Arabs and the Shilluk. This is unfortunate, as these systems are unilateral and recognise clan organisation and therefore, according to these new definitions, are closer to the "Joint Family" than to the "Kindred." Further confusion arises from the fact that the term "Kindred" is used, with the meaning given it in this book, to describe a certain form of grouping found on Eddystone Island, called *taviti*, which is of considerable importance in the social organisation of the island, regulating marriage, property, and punishment for crime, etc. It is a bi-laterally related group, there is no clan organisation on the island. Yet the relationship terms used by these people do not differ essentially from other classificatory systems, and Rivers considered that it closely resembled the Polynesian systems. Thus we have a group—in the present volume called a Kindred—which uses the Classificatory system of nomenclature and is morphologically related to

the Polynesian system, the latter being associated with clan organisation; and we have a Kindred System of nomenclature which is quite unrelated to the Kindred Group.

Nothing has been added to the somewhat tentative account of the Kindred System of Relationship given in *Kinship and Social Organisation*, i.e., that it is dependent on the extended family. Since the publication of that work I have been fortunate enough to study several of these systems at first hand, and, though I am not yet in a position to publish my results, I may say that I do not think that the morphological connection of this type of relationship system with the extended family, which he suggested, will hold good.

The chapter on property is mainly devoted to the different forms of tenure, individual and communal, and is of very great interest. Property in weapons, land and trees is dealt with. We can only regret that no attention has been given to hunting and fishing rights and that even still more interesting form of property—rights over other persons, i.e., slavery and property in women. The last mentioned is dealt with only indirectly in other parts of the volume. A connected treatise on this subject from Rivers's pen would have been of great value.

The chapter devoted to Government should be read with that on property not only for the interest of the subject, but in order to appreciate the attitude towards the study of anthropology that Rivers had adopted during the last few years of his life. He was much occupied with the change that he believed had taken place from the communistic to the individualistic mode of life, and concludes that this is not a simple evolutionary development, but has been brought about by the dominance of an individualistic people. He speculates that clan organisation, with its strong group feeling, may have been a *constant* feature in human development, though he admits the possibility of other alternatives. It will be remembered that, in *Instinct and the Unconscious* he suggested that agreements arrived at intuitively by communistic Melanesians were characteristic of the strong group feeling fostered by clan organisation. The same view is brought forward in this book; it is stated (p. 169) that:—“There is much reason to believe that this unwitting or intuitive method of regulating social life is, in many societies at any rate, closely connected with the communism which was considered in Chapter VI that among such a people as the Melanesians there is a group-sentiment which makes unnecessary any definite social machinery for the exertion of authority, in just the same manner as it makes possible the harmonious working of a communal ownership, and ensures the peaceful character of a communistic system of sexual relations.”

With regard to sexual communism, we find on page 80:—

“I should only like to emphasise again the fact that we have clear evidence that existing varieties of mankind practise sexual communism, and man must therefore have tendencies in that direction.” If we consider these tendencies are sentiments which can overrule sexual jealousy, I think few will deny that such tendencies exist whatever our views on sexual communism may be. Mr. Perry states in a footnote that, “there is no inherent necessity in this point of view. It is possible, on the contrary, that sexual communism could have arisen as a reaction to some social institution.” It seems important to draw the attention of psychologists to this point, as it is the focus of considerable theoretic interest, in which the editor's views differ from those previously published by Rivers and, as far as I can judge, from those expressed in the present work.

Rivers, in the works to which allusion has already been made, has elaborated a scheme of social organisation in which man, breaking away from a food-gathering state, developed a clan system with its accompanying classificatory system of relationship and strong group feeling. The dual organisation and communistic tendencies, both in its sexual and socio-political aspects, are considered to be essential to the

classificatory system. Immigrations of a highly civilised people took place, and the interaction of this culture with the more primitive types of culture produced the varying forms of society as we know them.

He considered the dual organisation itself to be the result of the fusion of two cultures—a phenomenon essentially different from the interaction of one people with another immigrant people of a higher culture who brought with them no women, or only a negligible number. And it is important to note that he has written—“ The greater part of my scheme takes the dual organisation as its starting point, and will hold good if my evidence for fusion is illusory and if the dual system is the result of fission.” (*History of Melanesian Society*, Vol. II, p. 590). While “ It is essential to the whole argument of this book that when Melanesia was first visited by the kava-people, its society was organised on the dual system with matrilineal descent.” (*Op. cit.*, p. 314).

To the kava-people the organisation of Secret Societies is attributed and the importance of this attribution to his theory of the spread of culture has already been noted. Rivers does not appear to have speculated on the beginnings of agriculture, though he has investigated the history and distribution of taro (*Colocasia esculenta*). However, there is nothing to show that he supposed that his “ dual culture ” lacked all forms of agriculture: and, from the general consideration of his work, it cannot be supposed that he assumed they were food-gatherers. Whatever the history of the “ dual culture ” may have been, Rivers considered that people who still possess this organisation, show a strong development of the gregarious instinct and that this is reflected in their institutions.

“ The fundamental and essential character of social structure ” was a principle of the first importance to Rivers and in the present work the emphasis has not changed: the interaction of cultures is still regarded as the main factor in bringing about change in social organisation and belief, and in this work Rivers ascribes the spread of civilisation to the migration of one people.

Mr. Perry's theory of dual organisation must be considered briefly in order to compare it with that of Rivers which has been indicated here, and on which his theory of social organisation rests. This theory of Dual Organisation, given in Appendix III, is arresting by reason of its comprehensiveness and simplicity. The scheme is “ historical ”; psychological and sociological factors are not considered. In the circumstances it is greatly to be regretted that no references to historical data are given and that such sentences as the following should occur (p. 210) “ that extensive gold-working and pearl diving *must have been prosecuted by these people* [the Children of the Sun] *in this region.* ” [Australia.*]

If I understand Mr. Perry aright, the Archaic Civilisation was in full swing in Egypt in the Sixth Dynasty and was characterised by the fifteen points given in Chapter XXV of *Children of the Sun*. Members of this civilisation scoured the earth in search of various precious commodities and formed colonies. Though it is not stated that this is so, it would appear that these colonists were members of the royal family (or, as he calls it, the ruling group). Everywhere they encountered people who had not passed beyond a food-gathering state, they became their rulers and taught them arts and crafts, and imposed upon them a form of government and a social organisation. In some places these people disappeared and then some or all the crafts were lost, and only certain beliefs, legends and the social organisation remained. The Dual Organisation with matrilineal descent and exogamy are three of the fifteen points that were carried *bodily by these adventurers*; *the former arose in Egypt owing to events* which occurred in that land over a period of about eight centuries. The first of these was the consolidation of Upper and Lower Egypt into one Kingdom,

* The italics are mine.

and the last the fission of the ruling group, owing to the rise to power of a family from Heliopolis.

This rendering of the Dual Organisation gives an entirely new meaning to the term and apparently ignores the close association between it and the classificatory system that Rivers considered necessary to his theory, as well as the tendency to communism which he considered to be derived from the gregarious instinct. Dual Organisation, according to Mr. Perry, took its final form in the Dynasty VI, but clan organisation was the outcome of the discovery of agriculture. Mr. Perry does not ask us to suppose that agriculture was not practised in Egypt before that date, so we must conclude that he considers the two institutions as independent.

But this is not the only point on which he differs from Rivers. Not only does Mr. Perry appear to deny the fundamental nature of social structure (*History of Melanesian Society*, Vol. II. p. 591), for according to him the classificatory system arises late in the history of a highly organised civilisation and is divorced by a vast period in time from the foundation of the clan; but he also disregards the interaction of cultures, for according to his theory clan organisation, dual organisation and exogamy were all carried bodily about the world and imposed upon peoples of a lowly culture. This sounds strange enough to the social anthropologist, but the psychologist will wonder whether gregarious tendencies can be dismissed as not being fundamental, and exogamy can so lightly be disassociated from primitive ideas of incest.

BRENDA Z. SELIGMAN.

REVIEWS.

Melanesia: Ethnography.

Fox.

The Threshold of the Pacific. By C. E. Fox. Litt.D. London: Kegan Paul, Trench, Trübner & Co., Ltd., 1924. Pp. 379. Price 18s. **34**

Those of us who were intimate with Dr. Rivers have long realised the high opinion he held of the researches of Dr. Fox, and he was very anxious that nothing should delay the publication of Dr. Fox's book. He himself, alas! was unable to do this service for his friend, but Professor Elliot Smith, assisted by Mr. Perry, has fulfilled the pious obligation entrusted to him by Dr. Rivers.

Through long residence in San Cristoval, Dr. Fox has had the opportunity of learning about the natives, and by his sympathetic attitude towards them he has been able to present an authoritative account of their social life and religious ideas and practices. This book is a mine of reliable information which will not only prove to be indispensable to those interested in the ethnology of the Solomon Islands, but will also serve to throw light on conditions in Melanesia generally. Out of the numerous subjects of importance treated by Dr. Fox, only one can here be alluded to.

Those who have read the preliminary account by Dr. Rivers (*J.R.A.I.*, LII, 1922, p. 16) of Dr. Fox's discovery of burial in pyramidal structures will naturally turn with eagerness to see what Dr. Fox himself says on the subject. On p. 219 a diagrammatic sketch is given of a *heo*, which shows a low quadrangular platform with sloping sides, that is, a truncated pyramid, described only here as a "Mastaba"; in front of this is a courtyard *hera*: on the top is a "dolmen" or *hau suru* and the orifice of a burial shaft which leads to a "burial chamber, into which the mummified body of the dead *araha* was lowered through the burial shaft and then placed on a bed" . . . later the jaw or skull was placed in the *hau suru* ("lifted up stone"). In the text we are told that the bodies buried in the *heo* were washed daily, evidently to remove the flesh as quickly as possible, and nowhere else is the term "mummified" used. We are also told by Dr. Fox that "other methods [of burial on a *heo*] were practised, for example, cremation"

(p. 219). In the detailed accounts of four *heo*, only the washing process is described. Cremation, which Dr. Fox believes was associated not with the *araha*, but with the latest immigration, that of the bird-totem people (p. 368), took place on a *hera* (p. 228), this is evidently a *heo* (p. 218). On p. 225 the process of embalming is described: one, a very imperfect process, consisted of covering the body, but not the face, with *o'a* shavings; the body was placed in a canoe which was slung over a small *heo*. "The body was sometimes painted with turmeric and kept fresh " for some time, allowing relatives from distant villages to view the dead." This was only for *araha*; no mention is here made of the body being placed in the burial chamber: this is the method "12. Burial in a canoe (embalmed)" mentioned on p. 217, and is distinct from the custom that everyone who is to be buried on a *heo* is first placed in a canoe, which has its ends cut off, and carried in it to the *heo* (pp. 210, 211). "Sometimes bodies put in *hohoto* [a long shallow wooden "trough] were embalmed." People placed in caves were sometimes embalmed differently. Two incisions were made, the viscera removed and buried, and the inside of the body was stuffed with *o'a* shavings. Thus the explanation of the figure on p. 219 unfortunately does not quite tally with the accounts in the text. It is to be regretted that no photograph, drawing or plan of a *heo* is given; the diagram referred to might very well have been constructed from the written description. With regard to the burial of the chiefs, we are told (p. 298) that it was "always extended, and always exposed: not interred; usually the skull " preserved. Burial on the *heo* and embalming are traditionally connected with " the *Araha*."

In the chapter on Stone Work, Dr. Fox brings together a number of interesting facts, most of which can be paralleled elsewhere, *hau maea* or sacred stones being abundant, spherical stones, *hau nagi*, are very common on *hera* (probably *heo*) and are much used for magic, they were sacrificed to. On one *heo* is a "turtle stone," on others "shark stones"; on some *heo* are rounded diorite stones which grow in size. A cup-shaped coral occurred on one *heo*; it was called "stone of the snake" as are other sacred stones on *heo*. Sacrificial stones were placed on *heo*. On some *heo* are stone pillars two or three feet high, of unknown use. On one *heo* a **J**-shaped stone was set up for the last man buried there and a simple rounded pillar for a woman, and also a strangely-shaped perforated slab of stone, which appears to have some meaning connected with marriage. Stones in which ghosts live or into which they go are placed on *heo*. Stone statues, occasionally of very large size, are placed on *heo*, but "the most interesting are the little stone statues [of diorite or coral], some only about a foot high, which used to be put on the *heo*, " of *Araha* at any rate, and into which one of the two souls possessed by a man " was believed to pass. . . . In some *heo*, bird statues take their place, and " in this case the male and female stones of the human statue *heo* are called the " 'bird's children'; they are set up by it, and are used for magic. . . . The " statues were painted in *Arosi* with turmeric after the men died whose *adaro* " would pass into them" (pp. 291, 292).

It is clear from the foregoing and from a mass of other evidence adduced by Dr. Fox that the members of the chiefly clan, the *Araha*, were a very important folk with a relatively high culture, which now shows traces of modification. We may assume that we are in the presence of a culture that, before it reached San Cristoval, was yet more highly developed. That elaborate cultures immediately from Indonesia, whatever their earlier history may have been, have penetrated into Oceania is admitted by everyone, and this is certainly one of them. Dr. Rivers went further and held that the evidence he then had from Dr. Fox "must make it " difficult for the most hardened sceptic to deny that it is at least a legitimate " hypothesis that there has been such spread and survival," that is, of "the

" transmission of Egyptian beliefs and customs to Melanesia " (*l.c.*, p. 17). Dr. Fox first became acquainted with Dr. Rivers in 1908, and as he was in constant communication with his " master," he has not unnaturally been largely influenced by the principles elaborated in " The History of Melanesian Society." Later he appears to have been attracted by the theory of the " archaic civilisation." In his Preface to " The Threshold of the Pacific," Professor Elliot Smith warns " the reader that another interpretation of the facts is possible than that adopted " by Rivers, and, following him, Dr. Fox." We wish that there were more workers in Melanesia who would give us as valuable material for the elucidation of the problems of the transmission of cultures as has been provided by Dr. Fox.

A. C. HADDON.

Britain: Archæology.

Stone.

The Stones of Stonehenge: A full Description of the Structure and its Out-works. Illustrated by numerous photographs, diagrams and plans drawn to scale. By E. Herbert Stone, F.S.A., Fellow of King's College, London. London: Robert Scott. 1924. Pp. 150 + xvi. 36 plates. 8½ by 10¾. **35**

In this handsomely produced book there is collected, from many different sources, information concerning Stonehenge dating from the sixteenth century to the present day. This is supplemented by chapters on the origin and nature of the stones themselves, with suggestions as to how they may have been transported, dressed, and set up in their present position. There is a generous supply of illustrations and plans. Of the latter, that made by Professor Flinders Petrie in 1880, and stated by him to be correct (as a plan) to within " about a thousandth of an inch," and representing the relative positions of the stones as they stand as being " in all " cases correct to within a quarter of an inch " has been adopted, together with Petrie's numbering of the stones, as the general reference plan for the whole book.

With regard to the chapters on the general design and dimensions of the stones of Stonehenge, and of that part of the monument lightly termed by the author as " the Outworks," it may be said that on all these points his book forms a handy compendium of the actual measurements and of the many opinions regarding Stonehenge held during the past 300 years. Some of these latter, expressed by Mr. Stone himself, have become obsolete even while he was compiling them. For example, on page 2 we read :—

" The present structure of Stonehenge, as we now see it, is all of one period. The erection of the sarsens is contemporaneous with that of the bluestones, the work having been continuous. This fact is fully established by the results of the excavations conducted by Professor Gowland in 1901, and by Colonel Hawley in 1920-21."

On page 65, however, presumably written a little later, we find this entirely contradicted :—

" There can be very little doubt that the original circle of bluestones as first erected at Stonehenge—long before the present structure was built—was simply a primitive circle of rough, unhewn boulders.

" We may reasonably suppose that the ' Aubrey holes,' recently discovered by Colonel Hawley, are the foundation pits in which the stones of this primitive circle were set up. The evidence of the fragments appears to show that when the present structure of Stonehenge was built, the older bluestone circle was dismantled, and the stones were then re-erected in their present positions."

Even this later opinion must now be at least modified in the face of the discoveries of 1923-24 on the site, particularly with regard to the finding of two new rings of

stone-holes within that of the "Aubrey holes," and concentric with it. There is still much more awaiting discovery—for example, whether or no the great "ring-fence," as we may imagine it, of dressed sarsens, with lintels, now covering little more than the eastern semicircle of the ruins, was ever actually completed to a whole circle; whether there ever were monoliths forming "opposite numbers" to the "Slaughter stone" and to the "Heel stone"; whether the side-lines of the "Avenue" ever were defined, as has plausibly been supposed, by two rows of standing stones; and, in particular, what the *facts* are respecting the "Altar stone" and the area at the heart of the monument which surrounds it?

When the whole story of these easily ascertainable particulars have been discovered by excavation, together with whatever other surprises the site has in store for us, and when the whole monument, earthworks and all, has been scientifically surveyed as a single object, and the plan of it, to the smallest detail, plotted on a sufficiently large scale, showing the True Meridian exactly determined, and laid down in correct reference to the positions of the earth-works and stone-holes of all ages, then will be the time to write a book about "Stonehenge" with any feeling of finality in it. In that respect the present volume has appeared too soon.

Chapter II, on the Age of Stonehenge, is a reprint of Mr. Stone's article in *The Nineteenth Century, and After*, for January, 1922.

In MAN of September, 1922, the present writer ventured to criticise adversely, on several grounds, the accuracy of the data arrived at in this article, and one of these criticisms—namely, the fact that there are (at least) two monuments, of different dates, on the same site—now appears to be justified, and throws into even wider fields of doubt the statement that Stonehenge is "probably not earlier than about 2040 B.C. and not later than "about 1640 B.C." If this dating is at all to be accepted, to which of all the monuments on the site does it refer?

Following the discoveries of the last few years, it now seems probable, in fact, that we are in the presence of three monuments; that is to say, of an original, with two separate "restorations" or augmentations, set up at widely differing periods of time.

First, we have the Earthwork Circle and Avenue, excavated by means of deer-horn picks, with which we may probably associate the rough untrimmed blocks of sarsen, of which a few still remain. Possibly the two newly-discovered rings of stone-holes and the "Aubrey holes" may belong to this stage.

Next, we have the introduction from South Wales (as is now certain) of a Circle and Cove of untrimmed Bluestones; and, lastly, the erection of the Cove of immense trilithons, with its surrounding lintelled Circle of trimmed Sarsen stone, and the contemporaneous trimming of the pillars of the Bluestone Cove.

All of these three structures, the Earthwork Avenue, the Bluestone Cove, and the great Sarsen Cove, have the same axial line (as reported by Sir Norman Lockyer and by Mr. Stone), indicating that the azimuth of the original "axis" was maintained faithfully throughout the succeeding structural changes. If this axial line is indeed the same for all the structures, and if its azimuth is that of sunrise at the summer solstice of the year when the *first* monument was laid out, what, then, is the date of the introduction of the Bluestone Circle, and what (almost most interesting) is the date of that surpassing engineer, named as "Merlin" in the traditions, who originated trabeated architecture in these hyperborean lands, and in the same moment of genius devised, for that "moment's monument," the mortice and tenon and the toggle joint, to keep all standing to futurity? A man, moreover, whose influence was such that he was able to command the services of multitudes on the previously unknown, and seemingly impossible, work of transporting 40 pillars of stone, each of them weighing (as Mr. Stone calculates) about 26 tons, and 35 lintels,

each weighing about seven tons, for, possibly, several miles; and then tediously to dress them with ponderous hammer-stones, and afterwards to set them up.

Who was this man, when did he live, and why is there only one Stonehenge in the world, without ancestor and without progeny?

We have to go as far as the island of Tongatabu, in the South Pacific Ocean, to find a trilithon (and that but a single one) at all comparable in size to those of Stonehenge. Quite as mysterious in its singularity—for it is the only one known in the whole Pacific region—it stands adjoining the tombs of the Tuitonga, or Sacred Kings of the island. It is constructed of coral blocks, cut out of the adjoining reef, and stands about 18 feet high. The lintel is not morticed, however, as at Stonehenge; but the top of each upright pillar is cut into, so as to form a square forked recess, in which the cross-beam rests.

As to the possibilities of transporting the great stones of Stonehenge, we have to go no farther than Egypt to find that such work, by human labour, was a common event of ancient times, and with even greater monoliths. Later still, in the third century of our era, an immense stone, weighing about 35 tons, was conveyed, by man-power only, for a distance of more than 80 miles from the sacred hill of Mihintale, near Anuradhapura, in Ceylon, to the then capital of Pollonaruwa, where it now lies, and bearing on it a long inscription in praise of King Tissa, is named "Galpota," or Stone Book.

Chapters III, IV, and V, on the Origin and Nature of the stones now remaining on the site of Stonehenge, with remarks on "dressing" them, will probably be found to be the most valuable ones, to those interested in the matter, in the book: dealing as they do, for the most part, with ascertained facts, and with careful experiments made by the author and others.

In Chapter VI interesting suggestions are made, also the result of experiments by the author, as to the means by which the great trilithons and the great Circle, with its lintels, may have been set in place. There seems little doubt that the stones *could* have been set up by the method proposed: but, if so, what has happened to the vast mounds of earth and chalk, etc., which must have been brought to the site for the purpose, of which not the least symptom remains, while the much smaller, and presumably much more ancient, earth-heaps of the vallum of the Ditch and of the "Avenue" remain still visible, if much diminished by wind, rain, worms and rabbits.

In conclusion, one notes with regret that even with so strong a protagonist for "Orientation" as the author, not one of the plans given in his book is laid down in such a way that the azimuth of any line, such as the "Axis" of the monument, shall immediately be evident—namely, so that the Meridian, the line indicating True North and South, shall be parallel to the edge of the book, with the North end uppermost, as in all scientific plans, including even school map-books. The importance of this is obvious, and much exceeds the ignoble excuse of economy of paper, which can be the sole reason for this crookedness.

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Britain: Archaeology.

Sollas.

To the Editor of MAN.

Late Palæolithic Art in the Cresswell Caves.

36

SIR,—No one could welcome the results of Messrs. Garfitt and Armstrong's exploration of Cresswell Crags more warmly than I do myself, especially as they

relieve the Aurignacian inhabitants of these islands from the unmerited reproach of an indifference to art. I only wish that they had been made in time for recognition in the last edition of "Ancient Hunters."

In the light of these recent discoveries the problem of the Cresswell "horse" assumes quite a different aspect and I feel all the more bound to offer an explanation of the statement for which I am responsible. It arose out of a conversation with the Rev. A. M. Mullins, Rector of Langwith-Basset, well known by his exploration of the Langwith Cavern, which is situated within easy reach of Cresswell Crags.

Happening to refer to the almost complete absence of any artistic work in the palæolithic deposits of this country, I mentioned the famous "horse" as the only known exception to the general rule, when Mr. Mullins at once interposed with the remark that this is no exception and proceeded to inform me that it had been surreptitiously introduced into the cave, more than one person—as I understood—having been concerned in this nefarious proceeding.

He demurred, however, and, as I thought very naturally, to my request for names, but assured me that he spoke of his own personal knowledge.

Any reflection on the good faith of any of the explorers of this cavern—particularly my old and dear friend the Rev. Magens Mello, the actual finder of the engraving, would have at once aroused my indignant resentment; but there was no hint of this, and as Mr. Mullins's statement not only disposed finally of what I had always regarded as malicious gossip, but was in general harmony with the state of knowledge at the time, I felt it my duty to make it public, even if only in a modest footnote. It is to be regretted that Mr. Mullins is no longer with us to add his explanations to mine. All I can do now is to withdraw the controverted statement and to delete the footnote at the earliest opportunity.

Perhaps I may be permitted to refer to another discovery which was made too late for notice in my last edition. It is of great importance, since it affects not merely a locality but a whole industry. I allude to the finding by M. Peyrony in a Solutrean layer at Les Eyzies, of a slab of limestone bearing a carving in high relief of two oxen (*Bos primigenius*).

When I examined this work last Christmas I was much impressed by its skilful modelling, fidelity to nature and artistic feeling. It recalls, though less bold, the famous bisons of Tuc d'Audoubert.

Hitherto the Solutrean age has afforded no objects of art and this has always been regarded as a remarkable fact, for the Solutrean people were the first to introduce that new method of working in flint which produced the most beautiful weapons of the Palæolithic age and afterwards culminated in the wonderful productions of Neolithic Egypt and recent North America. It was supposed, in explanation, that the Solutreans were a warlike, invading race who concentrated all their attention on their weapons and had none left for artistic effort. We now see how far this was from the truth.

Yours faithfully,

University College, Oxford,
10th March, 1925.

W. J. SOLLAS.

Correction. Dr. H. R. Hall writes to point out that in his article in MAN, 1925, 1, the excavations now in progress at Ur have been, by inadvertence, spoken of as if they were the work of the British Museum solely. On p. 2, line 29. *Museum's work* should read *Work of the Joint Expedition of the British Museum and the University Museum of Philadelphia.*

37

WINS
Clerical
F B I



FIG. 1.—BALLS OF CLAY FROM EL-AMARNA, ONE BROKEN AND SHOWING THE HAIR INSIDE.

FIG. 4.—MODERN CLAY BALL CONTAINING HAIR. 1 SCALE.



FIG. 2.—CHILD ABOUT TO HAVE THE TUFTS SHAVED OFF AT THE TOMB OF THE SHEIKH UMBAK.



FIG. 3.—THE TUFTS BEING SHAVED OFF. THE WOMAN SEATED NEXT TO THE BARBER ON THE RIGHT IS THE "SERVANT" OF THE SHEIKH UMBRAK.

ORIGINAL ARTICLES.

With Plate E.

Egypt: Religion.

Blackman.

An Ancient Egyptian Custom illustrated by a Modern Survival. 38
By Winifred S. Blackman. With Plate E.*

In the *Journal of Egyptian Archaeology*, January, 1915, pp. 8-9, with pl. IV, Professor Peet drew the attention of Egyptologists to a number of clay balls, such as those which are shown in pl. E, fig. 1. Those which he describes were "found in the sand filling of a small mastaba in Cemetery D at Abydos in the season 1912-13. The original number of the balls must have been about 40, and they lay mostly in the north-east corner (by river reckoning) of the sand core of the Mastaba (No. 124). In diameter they vary from 20 to 40 centimetres and they are all approximately circular" (p. 8). "Most of these balls have the impressions of a reticular seal, probably a cylinder." Three of these balls were cut open for examination, when two of them were found to contain fragments of reed (possibly papyrus), the third having within it a very small bit of linen cloth.

Professor Peet (p. 9) refers to the fact that similar clay balls were found by Professor Garstang at Reqaqha "in a small hole bored into the top of the wall of Mastaba 50, near its north-west corner."† These balls bear incisions similar to those found by Professor Peet at Abydos, but with additional incised drawings of animals and possibly a man. Mastaba 50 may perhaps be assigned to the Vth Dynasty, a similar or slightly earlier date being attributed to the Abydos Mastaba. Professor Peet concludes his note on these objects as follows: "It would be foolish to dogmatise with regard to the purpose of these objects. That they were connected with the rites of burial is, however, fairly certain, and we may conjecture that they had some magical significance" (p. 9).

A further reference to these balls is made in a note in the *Journal* of October, 1915, pp. 253-4, where Mr. Griffith suggests that some reference to contracts with the priesthood may have been inscribed on the pieces of papyrus or cloth found within these particular balls. The same authority goes on to say: "The tomb contracts were evidently a source of much quarrelling and anxiety, and I should think these symbolic contracts may have been deemed to have magic force for the fulfilment of the originals—but many alternatives are possible."

Again, in the *Journal* for April, 1916, Miss Crompton publishes a note on "Two clay balls in the Manchester Museum," the accompanying Pl. XVI figuring the two balls in question, each cut in half and thus exhibiting their contents. These balls, like those mentioned above, are made of clay, but the outer surface is smooth, without incisions or inscriptions. They were discovered by Professor Sir Flinders Petrie in a tomb at Kahun in 1890, and, when presented to the Museum by Mr. Jesse Haworth that same year, they were unbroken. In 1916 the museum authorities decided to cut these balls open, and found that each one contained "A tuft of red-brown human hair, apparently infantile!" The tomb group to which these balls belong is assigned by Sir William Flinders Petrie to "about the XXth Dynasty," and Miss Crompton, having referred to the discoveries of the very similar clay balls by Professor Peet at Abydos, goes on to say that "It is singular that the dated examples known should be separated by so wide an interval of time as that between the Old Kingdom and Dynasty XX, even though the contents are entirely different."

* The funds available for my work in Egypt during the last season were largely provided by the Trustees of the Percy Sladen Memorial Fund, and by supplementary grants from the Royal Society and the Committee for Anthropology in Oxford.

† Garstang: *Third Egyptian Dynasty*, pp. 32, 59, Pl. XXX.

In *The City of Akhenaten*, Part I (an account of the excavations conducted by the Egypt Exploration Society in 1921 and 1922), p. 66, it is stated that "A quite common object was a small ball of mud, sometimes stamped with the impressions of signet-rings, containing a wisp of hair: they may have been dedications, a lock of a child's hair being vowed to a god in the event of his reaching puberty—a practice still current in some districts of Upper Egypt—or they may represent a more malevolent type of domestic magic."

This year (1924), among the finds from El-Amarna exhibited by the Egypt Exploration Society, were two or three clay balls, discovered during the excavations conducted on that site by Mr. Newton, on behalf of the Society. The Society has most kindly permitted me to refer to these specimens to illustrate his paper, and has furnished me with the photograph which is reproduced here (Pl. E, Fig. 1).

While continuing my researches last winter among the *fellāhīn* of Upper Egypt I came across the following custom, and also had the opportunity of witnessing the ceremony connected with it. In all Egyptian villages a number of little boys may be seen with their heads shaved, save for a few tufts of hair which are left untouched. I am told that each of such tufts is dedicated, in the case of Muslims, to a sheikh, or to a saint if the child's parents are Copts, or sometimes all the tufts are dedicated to one sheikh or saint. I am told that there is no *special* age for the hair to be cut in this way, but I believe it is always done in very early childhood, for I have never seen any but young children with these tufts of hair left on their heads. Contrary to the suggestion put forward by Professor Peet, the ceremonial cutting off of the tufts is not, so far as I have discovered, performed at any particular age of boyhood, but the date of the performance seems to depend on whether the father can afford the expense that the ceremony entails, such expense, in some parts of Upper Egypt (Asyūt Province, for example), being considerable. It has never been suggested to me that the ceremony takes place when the boy has reached the age of puberty. Indeed, as far as my own experience goes, the tufts were removed long before that period was reached.

When it is desired that the tufts should be shaved off, quite an important ceremony takes place.—at least such is the case, according to my informant, in Asyūt Province. When possible, the child is taken to the tomb or tombs of the sheikh or sheikhs to whom the tufts of hair are dedicated, and the tufts are removed just within, or just outside, the sacred building, or the child may be taken to a mosque where the ceremony also can be performed. If the child is a Copt it is taken to the church or churches named after the saint or saints to whom these tufts of hair have been dedicated. In the case of the Muslims the village barber is called in to remove the hair, but with the Copts a priest performs the rite. The removal of these tufts takes place if possible when the hair is four to five centimetres long. But in parts of Upper Egypt the ceremony is so expensive for a poor *fellah*, that the child may have to wait some time longer before the tufts can be removed. Bread and meat are taken to the tombs or mosques where the ceremony is to be performed, and then a feast is held to the accompaniment of dancing, beating of the drum (*darabukka*), hand-clapping and *zagharīt*. The barber on these occasions may demand quite a large sum of money, and altogether the ceremony rarely costs less than six pounds, often more.

I myself witnessed the performance of the rite in Fayūm Province, where the proceedings were simpler. The child whose tufts were to be removed was taken to the tomb of the Sheikh Umbarak, to whom they had all been dedicated (Pl. E, Fig. 2). The barber arrived and shaved the hair off just within the entrance to the tomb (Fig. 3). On this occasion the child was held by the sister-in-law of the child's mother, but I do not think there is any fixed rule as to who should

act in this capacity. There was no feast or dancing, but such festivities may take place sometimes in this province, though they are not necessary, as I was told was the case in Asyüt Province. The child's mother is a friend of mine, and as I paid the barber's fee myself, I managed, with the assistance of the woman who held the child, to secure half the hair that had been cut off. The cut-off hair is always buried outside the tomb or mosque where it has been cut off, and is put in the ground either loose, or else is first enclosed in a clay ball. On the occasion when I was present at this ceremony the hair was enclosed in a clay ball and buried outside the Sheikh Umbarak's tomb, and half was kept back and put in another clay ball and presented to me. The ball in my possession, of which a photograph is reproduced here, is somewhat flattened at the base, owing to the fact that the clay of which it is made was rather moist, and the placing of the ball outside the house to sun-bake it naturally flattened that portion which rested on the ground. (Pl. E, Fig. 4.)

As stated above, Coptic parents take their children to have the tufts removed at the church or churches, or sometimes it may be the tomb or tombs, of the saint or saints to whom the tufts of hair are dedicated. The priest, as has also been stated, officiates as barber, the father presenting a gift of money to the church or churches where the tufts are removed. On such an occasion a special service is held at the church, and oxen and fowls are killed outside the building, the meat, together with bread, being distributed among the assembled crowd of friends and the poor. If the parents are not rich enough to supply such a munificent feast, they will bring a smaller quantity of meat with them and will distribute it and bread to the crowds which always collect outside the building on these occasions, but in such cases no sacrifices are made. Among the Copts no dancing takes place at this ceremony, as among the Muslims, but the women give the cries of joy (*zaghari*) and there is a good deal of singing.

I have been told that if a woman remains for a long time after marriage without the prospect of a child, she will pray for a son, promising that, in the event of her prayer being answered, she will have the boy's hair shaved, leaving only the tufts, which she will dedicate to the sheikh or sheikhs, or to the saint or saints, with whom she has interceded. When offering such prayers, she will probably repair to the tomb of the sheikh, or to the church of the Coptic Saint to whom she wishes to appeal. I was told in Fayüm Province that if a boy is seen with his hair cut into tufts it means that he is an only son, and that this style of headdress will ensure him a long life. The initial cutting of the hair, it should be pointed out, is performed at home.

This modern custom may explain the clay balls found on ancient sites in Egypt, at any rate those found containing hair, which hair, as stated by Miss Crompton above, is "apparently infantile."

A common form of votive offering to be found hanging up in sheikhs' tombs at the present day is a small piece of the dress of a devotee. The offerings are suspended from a cord or cords which hang over the catafalque inside the domed building usually erected to the memory of a dead sheikh and in which the holy man is often buried. Possibly the small bits of linen found in some of the clay balls represent an early form of votive offering.



FIG. 1.

WINIFRED S. BLACKMAN.

Spain: Archæology.

Hildburgh

Notes on an Iberian Bronze Votive-Figure and on an Iberian Gold Earring. By W. L. Hildburgh, M.A., Ph.D., F.S.A. **39**

FIG. 1.—IBERIAN BRONZE VOTIVE FIGURE.

The little figure (present height $3\frac{1}{2}$ in.) of a so-called priestess* here illustrated is an unusually good example of a particular form of votive offering of which a number of specimens are already on record.† It was obtained some years ago at Burgos; unfortunately, no information about where or how it had been found was attached to it. It represents an Iberian woman, her hands stretched forward and inclined downward, in a close-fitting garment reaching her ankles, with her characteristic mitre covered in part by a long veil, and with her bare feet set upon a thin circular base. From her neck hang torcs which, seemingly (the small scale of the figure, and its somewhat sketchy treatment, make cautious judgment advisable), are of the smooth type found mainly in the north of the peninsula, rather than of the type, formed of twisted strands, found mainly in the southern parts and sometimes, seemingly, represented on little bronze votive-figures from these parts.‡

The base of the figure is, in figures of this particular kind, so exceptional a feature—none of the others I have alluded to above

possess it—as, considered in conjunction with the locality whence the figure lately came, to suggest that the latter was found in some sanctuary other than those in Southern Spain whose sites have up to now been investigated.

The earring, also, was obtained at Burgos, and similarly without any indication whatsoever either of the locality or of the circumstances in which it was discovered. Its type is one, perhaps inspired by a bunch of grapes§, of which several examples from the province of Granada have been recorded; none are known definitely to have

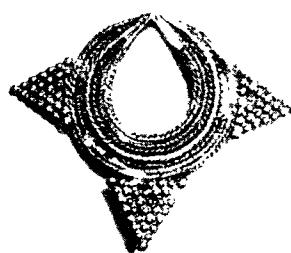


FIG. 2.—IBERIAN GOLD EARRING.

* On the dress represented in Iberian votive-offerings, cf. J. Pijoan, "Iberian Sculpture," in *Burlington Mag.*, Vol. XXII (1912), pp. 73 seq.

† Cf. P. Paris, "Essai sur l'art et l'industrie de l'Espagne primitive," Paris, 1904, Vol. II, pl. III, pp. 193 seqq.; H. Sandars, "Pre-Roman Bronze Votive Offerings from Despeñaperros" in *Archæologia*, Vol. LX, pl. XII, p. 86; J. R. Mérida, in *Revista de Archivos*, Vol. IV, pls. V, and VI, pp. 155 seq., and Vol. VII, pl. VI, p. 274; I. Calvo and J. Cabré, in *Memorias de la Junta Superior de Excavaciones y Antigüedades*, Madrid, No. 1 of 1916, pl. XV, and *ibid.* No. 1 of 1918 (general No. 22), pl. XII; F. Almarche Vásquez, *La antigua civilización ibérica*, Valencia, 1918, pl. facing p. 154.

‡ For some notes on Iberian torcs, and on their occurrence on votive-figures, see Hildburgh "A Find of Ibero-Roman Silver at Cordova," in *Archæologia*, Vol. LXXII (1922), pp. 168 seq.

§ The resemblance may, however, be only a chance one; the use of grains of gold such as have been employed here, lends itself to dispositions of this kind. Many examples of similar triangular groupings are to be seen figured in M. Rosenberg, "Geschichte der Goldschmiedekunst," part on "Granulation." Frankfort a./M., 1918.

been discovered in the district ordinarily paying archæological tribute to Burgos. A very similar object, also of gold, has been figured (on Pl. XVII) by J. Cabré and F. de Motos, in *La Necrópolis iberica de Tútugi (Galera, provincia de Granada)*, Memoir No. 4 of 1918 (general No. 25) of the Junta Superior de Excav. y Antig., Madrid (1920). Described as being finely worked, it differs from the present example in having its side-extensions only two rows deep, instead of triangular: it has been listed amongst small objects of "estilo egipciante greco-fenicio-punico." Several smaller pieces of the same kind, but lacking the side extensions, found in the necropolis at Tútugi, have been figured on the same plate.

W. L. HILDBURGH.

Britain : Archæology.

Stone.

The Purpose of Stonehenge. *By E. Herbert Stone, F.S.A.*

40

We know something about Stonehenge, and there is a great deal more that we do not know. But of all subjects connected with this remarkable structure there is none which has a greater interest for most people than the problem of its purpose. Generally the first question asked by the casual visitor to Stonehenge is: "What was it for?" Eminent archæologists have debated the same question for many years.

Stonehenge must have been a work of considerably more importance than any structure of its period in Britain. There is, perhaps, no edifice in the world which has been the subject of more discussion, both by the learned and the unlearned: but the purpose for which it was constructed will probably for ever remain unknown.

There has, however, been no lack of conjecture, of which the following are examples:—Astronomical observatory, Calendar for the seasons, Temple for some form of worship, Headquarters of a priesthood, Sepulchre for chieftains. Sepulchral use connected with barrows, Memorial hall or monument of victory, Palace for a great king, Hall for ceremonial meetings of chiefs, Court of justice, Theatre for contests or ordeals, To rival or supersede Avebury.

Of these conjectures the two which hitherto appear to have found most favour with archæologists are:—

(a) that Stonehenge was designed for some purpose connected with the observation or worship of the Sun; and incidentally as a calendar for the seasons.

(b) that Stonehenge was designed for some sepulchral purpose connected with the adjacent Barrows.

The evidence in favour of the former is the fact that the axis of the structure is directed (as nearly as can be ascertained) to the point on the horizon at which midsummer sunrise occurred when Stonehenge was built.

In July, 1920, a meeting of the Wiltshire Archæological Society and the Royal Archæological Institute was held at Devizes. In the official handbook issued by the Society on that occasion for the information of their visitors, the following remarks are made regarding the probable purpose of Stonehenge:—

"The evidence seems to point irresistibly to the conclusion that it was planned and orientated in connection with the observation of the Sun, and consequently in all probability with its worship."

And regarding the direction of the axis of the structure to the point of midsummer sunrise, continues:—

"It can scarcely be maintained that this circumstance is likely to have been due to an accident, and, although it is capable of various

“ interpretations, the fundamental idea based on the observation and
 “ veneration of the Sun (and also perhaps of other heavenly bodies) remains
 “ the same.”

Turning now to a consideration of the “sepulchral purpose” theory, we find that twenty-five years ago, at the end of the last century, the opinion commonly accepted was that Stonehenge was of Bronze Age date, and that it was constructed for “some sepulchral purpose” connected with the adjacent Round Barrows.

During the last twenty years, however, the facts ascertained by the excavations carried out by William Gowland (in 1901) and the astronomical computations of Norman Lockyer have been gradually assimilated. Fresh light has, moreover, lately been thrown on the subject by the excavation work undertaken by Colonel Hawley. The result is that at the present time (1924) it may be regarded as practically certain that the structure of Stonehenge, as we now see it, is of Neolithic date.

Of the 306 barrows now remaining within a circular area of 20 square miles around Stonehenge we find that 294 are of the Bronze Age. Hence it would appear probable that when Stonehenge was built, 96 per cent. of the barrows now to be seen in its vicinity did not exist.

But even if Stonehenge and the adjacent barrows were of the same period and the work of the same race, it would, of course, by no means follow that they were co-related. People died and were buried in London in Norman times—but no one supposes that this fact has any connection with the building of Westminster Hall!

The writer has prepared a map showing by different shades of colour the relative density in the distribution of barrows in different districts over the whole of Wiltshire. It is apparent therefrom that the districts over which the barrows are most numerous are precisely those which would have been most likely to have been selected by the Neolithic and by the Round Barrow people as most suitable for occupation. It is found, moreover, that parts outside the chalk areas are left almost entirely blank, indicating that those parts were practically uninhabited.

We can scarcely suppose that these people deliberately selected the most desirable habitation sites for the burial of their dead, and then went to live elsewhere. We may conclude, therefore, that these people generally interred the remains of their dead in the neighbourhood of their dwellings, and that where barrows are found to be most numerous, there the district had been most closely populated or had been inhabited for the longest period. As James Fergusson remarks in this connection:—

“ They [the Barrows] are scattered either singly or in groups so completely without order, that the only feasible explanation seems to be, that each man was buried where he lived; it may possibly have been in his own garden, but more probably in his own house It may have been that when the head of a family died, he was buried on his own hearth, and an earthen mound replaced the hut in which he lived.”—*Rude Stone Monuments*, p. 102).

The chalk area in Wiltshire is about 900 square miles. The number of barrows of all kinds marked on the six-inch-to-a-mile Ordnance map is 1,383, giving an *average* distribution of rather over one and a half barrows to a square mile.

The distribution, however, varies considerably. Taking an area of four square miles as a convenient unit, we find, on what were then apparently the most closely-populated districts in Wiltshire, the greatest number of barrows (marked on the Ordnance map) which may be contained within a rectangle of four square miles to be as shown on the table below:—

Selected areas of densest population.	Number of Barrows.	
	Maximum on four sq. miles.	Average per sq. mile.
North and South of the Cursus, partly in Amesbury parish and partly in Durrington	137	34.2
About five miles to N.E. from Stonehenge, partly in Milston parish and partly in Bulford	96	24.0
About $3\frac{1}{2}$ miles to S.W. from Avebury in northern part of Bishop's Cannings parish	71	17.7
About a mile north of Sidbury Hill and $1\frac{1}{2}$ miles S.E. from Everley	43	10.7

In the neighbourhood of Stonehenge the most closely-populated districts (as indicated by the barrows) appear to have been in groups of settlements located on suitable sites towards the north and west. There is nothing in the arrangement of the barrows or the position of the sites to suggest any intentional connection with Stonehenge, nor is there anything in the design of Stonehenge itself to indicate any "sepulchral purpose."

Within an area of 20 square miles around Stonehenge the number of barrows of all kinds now existing is 306, giving an average of 15.3 barrows to a square mile.

In the immediate neighbourhood of Avebury there are comparatively few barrows, scattered more or less indiscriminately, and here also we observe that there is nothing to suggest any intentional connection with the stones. The nearest closely-populated district (as indicated by the barrows) is about $3\frac{1}{2}$ miles to the south-west, in the northern part of the parish of Bishop's Cannings (see table above).

Within a circular area of 20 square miles around Avebury the number of barrows of all kinds now existing marked on the Ordnance map is 94, giving an average of 4.7 barrows to a square mile.

The foregoing remarks have, of course, their widest application in reference to the Round Barrows, which (as now existing) are considerably more numerous than the Long Barrows—the ratio in numbers being about 20 to 1. The Round Barrows are commonly in groups of a dozen or more, as if all belonging to one family or clan. The Long Barrows are generally found singly, as family sepulchres, standing alone. But as regards distribution of population the remarks made above may be taken to apply equally to both classes: for we find, as might be expected, that the districts which had apparently been considered by the Neolithic people as most suitable for their settlements were also adopted by their Bronze Age successors.

From the considerations as set forth above we may apparently draw the following conclusions:—

(a) That where barrows are found to be more numerous this is simply an indication that the district was more closely populated, or that it had been inhabited for a longer period.

(b) That, although barrows are very numerous in the neighbourhood of Stonehenge, there is no indication that they were in any way co-related with that structure.

(c) That there is no reason to suppose that the remains of chieftains were brought from a distance to be interred in the neighbourhood of Stonehenge, or that Stonehenge was erected for any sepulchral purpose.

In regard to the design of Stonehenge, it has been supposed that this may have been evolved from the primitive "Stone Circle": but, except that the peristyle of Stonehenge happens to be circular in plan, there is absolutely nothing in this highly-specialised architectural design which has anything in common with a Stone Circle. The leading idea of the stone circle is simply a boundary or enclosure, and the same expression survives in the present day in the circle of stone (or cast iron) posts surrounding a market cross—a clock tower—or a monument.

If we want to imagine some remote prototype for Stonehenge we might with more probability suggest the circular dwelling-hut. It appears, however, more reasonable to suppose that this grand Structure was a definite design, and that it was made circular in plan simply because that form was found suitable for the purpose of the building and the conditions of the site. The general appearance of this great work expresses a similar architectural idea to that more fully developed in the Colosseum at Rome, and the internal arrangements have points of similarity with those of Buddhist temples in India.

Next, as regards the circumstances connected with the building of Stonehenge.

Within a circular area of 20 square miles around Stonehenge there are twelve Long Barrows marked on the Ordnance map. This gives a ratio of between nine and ten times the number per square mile as compared with the average (for Long Barrows) for the whole of Wiltshire. In Neolithic times, therefore, the neighbourhood of Stonehenge must have been a very important district, in which there were no doubt several well-populated settlements, as evidenced by these great sepulchres erected for their chieftains.

When it was decided to build the present structure of Stonehenge the conditions in the neighbourhood were probably somewhat as follows:—

The Neolithic people had already occupied the district for some thousands of years. Tribal wars between the dwellers on the plain had ceased, and the country around was at peace. The ditch surrounding the early fortified settlement on the Stonehenge site had long become unnecessary for defensive purposes, and was now nearly silted up. The site itself had been abandoned for habitation, and the people had made new settlements in convenient positions on the open plain outside. There was now a considerable population in the neighbourhood, so there was no difficulty in obtaining the large number of labourers required for the works connected with the building of Stonehenge.

When Stonehenge was built the district was thus a sort of metropolis for the Neolithic people in this part of Britain, and a convenient centre for religious or civil meetings of the allied tribes or clans. It would, therefore, naturally be selected as the most suitable place for the erection of a great public building.

Stonehenge was undoubtedly the greatest and most magnificent structure of its period in Britain, and we may regard it as most improbable that it could have been erected for any local or (so to speak) parochial purpose, merely to serve a few hamlets in its immediate neighbourhood. It is only reasonable to suppose that it was designed to serve some purpose of national importance for all the allied tribes in this part of the country—such as a great Temple for some form of worship—a Court of justice—or a Hall for ceremonial meetings of the chiefs.

E. HERBERT STONE.

Africa, East: Ethnology.

Crabtree.

The Origin of the Bahima: Supplementary Note. By the Rev. W. A. Crabtree.

In my paper under this title (*J.R.A.I.*, liii, p. 486, *inf.*) I alluded to a chief whose title is Gabunga. Further study enables me to supplement and correct as follows:—

Gabunga, chief of canoes. The word contains two elements, viz., "ga-bunga." *bunga*, canoe, occurs in J. 189, *mbungu*; J. 148, 187, *bongo*. The base is BUL for BAL, hollow, which is seen in a fairly representative number of words for "valley," in Bantu. BUL is closely allied to GUL, hollow, as in Cw. *mo-gogoro*, a valley; *kgoro*, hollow; *mo-koro*, a boat; and from GUL derive nine words for canoe in JCS, whilst the form LUG is given in about nine more and LUB in two or three more. The significance of this I refer to later, as also the change of final vowel from "u" to "a" to designate a title.

ga, prefix, is not easy to explain as a Bantu form. It is probably non-Bantu and derived from $\sqrt{\text{MAG}}$, master owner, akin to Ind. Eur. $\sqrt{\text{MAGH}}$. MAG is found in Ny. *mukama*, king, master, owner (Maddox), and G. *mukama*, master, owner (Pilkington). In Hausa it becomes the prefix *mai*, or *ma*, owner of, or doer of—*e.g.*, H. *mai-gona*, owner of a farm; for the nomen agentis Hausa mostly changes the final vowel to "i," as Bantu does—*e.g.*, H. *ma-karanci*, a scholar (H. *karanta*, to read), equivalent to G. *omu-somi*, one reading. Hence Ga-bunga, master or owner of canoes.

Note also that the Gabunga is *not* one of the ten landed chiefs under the King of Uganda. He holds a more or less independent position.

As a title, Gabunga derives from some earlier day, when, as my paper endeavoured to show, the ruling class lived in what is now Azande country. Moreover, the aristocracy was of Asianic or possibly Hittite origin. Autran's definition of "Asianic" as the people from Asia associated with the Semitic traders we call Phoenician is useful for our purpose. It helps to explain how an Ind. Eur. root came into Africa.

When we find from analysis of Hausa words that the phonology of Hausa is Bantu, that non-Semitic Hausa words are partly Bantu and partly Ind. Eur., especially Greco-Latin with a dash of Celtic, a further field of study opens out. To supplement my paper I might add that from an analysis of Hausa words it seems as though the ancestors of the Hausa lived as a trading community adjacent to a Bantu kingdom: their craftsmen Asianic, their organisers partly Semite, partly Asianic. These conditions could be fulfilled by a Bantu kingdom of Agi-Sumba on the one side, the Phoenician colony of Carthage on the other, and an extensive overland trade in the hands of an intelligent trading community in between.

As space does not allow of an amplification of these matters, I can only add a brief note of a few points that seem immediately important.

(i) The ending "a" for a title in Ganda is paralleled by the words for "god"—viz., G. *katonda*; Ny. *ru-hanga*; Sg. *ki-bumba*. The second element in each case is a verb, whilst the prefix of the first and the last is most likely this element GA, owner, master, and not a Bantu prefix at all. Now, in Sg. *ki-bumba* we have the common B. *bumba*, mould clay; in the others G. *tonda*, usually said to mean "create," but according to Pilkington, "be conceived, of the first beginnings of conception," and G. *wanga*, Ny. *hanga*, to put a blade in its handle (so "God distributes to each his work").

Therefore every one of these words, if made into a nomen agentis, as "creator," "distributor," would have been of the form *omu-bumbi*, a potter, with "i" ending,

and they are not. They are titular and formed in prehistoric days with a non-Bantu prefix.

(ii) Further, these words occupy a personal class of their own; if a plural is required it is usually BA, as *ba-katonda*, gods. To this day a few foreign words, like *supu*, Eng. soup, which do not conform to Bantu ideas of classification, go into this class.

In addition, in this class are a few impersonal things like *kaumpuli*, the plague; *kabotongo*, syphilis; *kawāli*, small pox, with *ka* prefix, and some ten others without prefix. This *ka* prefix may stand for GA and answer to H. *ma*, a prefix sometimes used in Hausa for concrete nouns—e.g., H. *ma-tsoraci*, fear. H. *tsoro*, to fear.

(iii) The ending for Bantu words for "canoe" is normally "u" or "o": "u" denotes condition, "o" is gerundal, or, if unstressed, short "ö" can be found, it would arise from the corresponding unstressed short "ü." This third alternative does not occur in words with which I am personally acquainted. The ending therefore signifies "hollow condition" or "being hollow."

(iv) The comparative rarity of the forms for "canoe" given above (some 20 instances only out of the 256 given in JCS.) suggests that if there was a large kingdom nominally Bantu but impinging on many non-Bantu elements, then *bunga*, canoe, was pronounced dialectically in at least three ways. Ignoring for the moment the prefix, these would be *bungu*, *lungu* and *lumba* (for *lumbu* with dissimilation). At this day Kele normally replaces Bantu *mb* by *ng*, whilst a few sporadic instances of this change can be traced in other Bantu languages. Further, the interchange of *l* and *b* is as old as time, and can still be found in Bantu. Zande usually changes Bantu *m* into *ng*: hence Zd. *ku-lungba* for Homa *ku-lumba*.

(v) For developments of Ind. Eur. \sqrt{KU} , to be hollow, we have a form KUL in Gr. *κοῦλος*, hollow, and this answers to GUL above. Also a form KUB in Skt. *kūpa*, hollow, Gr. *κύπη*, a hollow, etc. Whilst in Bantu we have Sw. *ki-kombe*, a cup, and a few derivatives for "canoe," as Sw. *ntumbwi* (**ntumbu*).

(vi) By contrast it should be noticed that the Bantu form is GAL, hollow—or perhaps GA—developing as GAT for "canoe" and GAT or GANG for "palm of the hand." In any case the characteristic vowel of the Bantu series $\sqrt{GA(L)}$. \sqrt{BAL} "hollow" is "A" and of Ind. Eur. \sqrt{KU} , mostly "U."

W. A. CRABTREE.

Britain: Archæology.

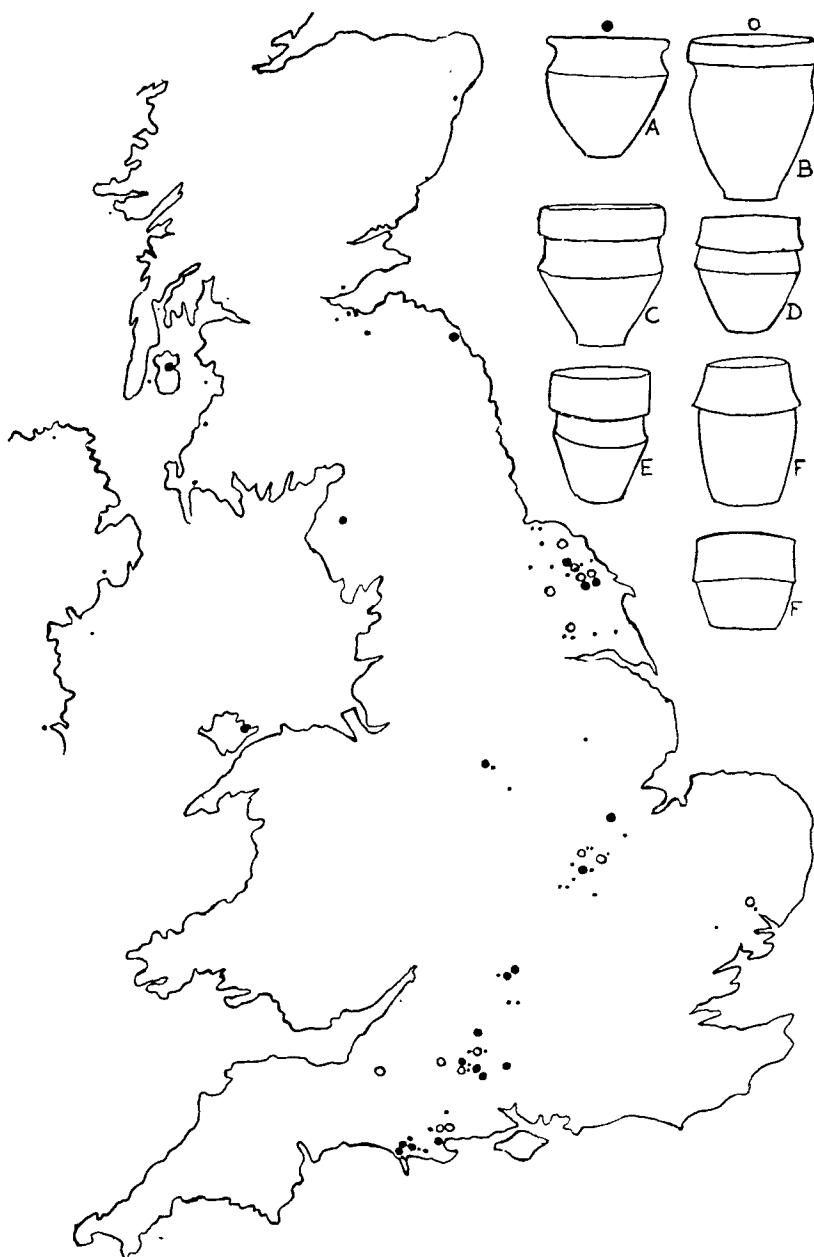
Petrie.

The Entry of the Bronze Users. By Sir Flinders Petrie, D.C.L.,
F.R.S., F.B.A. **42**

Some years ago I made a suggestion to the late Lord Abercromby as to the course of entry of the Bronze Age invaders, as I did not wish to interfere in his own subject. As the chance of the matter being followed up with his special knowledge is now, unhappily, past, I would state the case here.

The cinerary urns show a variety of depth of brim which appears to degrade from an ordinary lip, as A type (see map), until at last it swallows all the neck down to the shoulder, as F type. These varieties may be definitely graded according to the proportion of the height of lip to the height from shoulder upward (see types A to F). For the purpose of tracing the stages of entry of the Bronze Users the early forms are needed alone. The A type is marked on the map with a large spot •, the B type with a ring •, the C type with a dot. The obvious result is that the invaders did not attempt the south-east of England, but came in simultaneously at Weymouth, Poole, perhaps up the Avon, at the Wash and in east Yorkshire. The Wash invaders pushed up Northamptonshire and probably joined with the

Wiltshire group through Oxfordshire. All of these made urns of the first type. There are also very significant examples of type A in Arran and Anglesey. Evidently the invaders had good shipping, which could work round Land's End, and they may have preferred islands in order to be unmolested.



The early absence of S.E. settlement was continued, as there was only one example of type D in Surrey and one in Sussex, and none later, nor any in Kent. The conclusion must be that the good land of the south-east was strongly held by Neolithic folk, and newcomers had no chance there; while Dorset, Wilts, and Yorkshire were less occupied, being heaths, downs and wolds.

When we compare the incised patterns with these types, it does not seem that there is any connection of period. This means that the patterns were not copied from pot to pot, but were taken direct from basketwork throughout.

On referring to the objects found with those cinerary urns, with type A were a flint arrowhead, bronze knife and pin, jet pendants; with type B, worked flint, gold beads and pendants, amber beads, bronze knife and awl; with type C, worked flint, gold beads, amber beads and bronze knife. The bronze knives were, therefore, brought in at the beginning; the gold objects with B and C show that skill in fine work was brought in, as the activities of conquest and travel would not favour invention in the A period.

FLINDERS PETRIE.

REVIEWS.

Italy: Archæology.

Randall-MacIver.

Villanovans and Early Etruscans: A Study of the Early Iron Age in Italy as it is seen near Bologna, in Etruria and in Latium. By David Randall-MacIver. Oxford: Clarendon Press. 1924. Pp. xx + 270 + 46 plates. Price £4 4s.

43

Just as the historic civilisation of Western Europe begins in the Roman Empire, so during the Bronze and Early Iron Ages the chief cultural influences affecting Northern and Western Europe were mediated by Italy. Hence the prehistory of the Apennine Peninsula is not only of local interest, but is bound up to a peculiar degree with the study of our civilisation as a whole. Professor Peet's great book has already made the results of archæological study of the Italian Bronze Age available to English readers. Now Dr. Randall-MacIver begins to carry on the story into the Iron Age with this handsome monograph on the Central Italian material, to be followed, we are glad to learn, by another volume. The book before us is easily the most important contribution to the subject that has yet been made, and forms an indispensable aid to the study of the Iron Age in general. All the available material necessary for the comprehension of the period in Umbria, Etruria, and Latium has been here collected, sifted with admirable judgment, pleasantly described and bountifully illustrated with fine reproductions of all the leading types. In marshalling the facts the author uses the typological method with sound caution. He favours a relatively high dating without going to the extremes advocated by Montelius. Indeed, the latest results from transitional sites in Greece such as Thebes, Kephallenia, and Vrokastro might allow Tolfa and Allumiere to be pushed back to the thirteenth rather than the twelfth century, as here proposed. But foreign comparisons are throughout very charily used. Indeed, Dr. Randall-MacIver has exercised an almost incredible self-restraint. He has set himself to give us an intensive account of the Villanovan and Early Etruscan material of Italy and keeps strictly within these self-imposed limits. The material itself obviously struggles to burst these narrow bounds. But, of course, once you start pursuing an Iron Age form across the Alps, you will find yourself in the Caucasus before you know where you are! Yet sometimes a little latitude might have been allowed us. When contending that the Villanovans came from Central Europe, it would have been easy to cite the good prototypes for the typical ossuary to be found in the Bronze Age pottery of Slavonia and Hungary. In dealing with Etruscan chamber tombs a reference to the newly-discovered necropolis of S. Andrea Priu in Sardinia would not have been altogether irrelevant. An occasional pointer to material of this nature would surely illumine the immediate facts by giving them context and background.

A further consequence of the author's aim is that the interpretative sections are reduced to a minimum. What the average reader would like is no doubt to hear how the author would correlate the archæological material he has ordered in

such a masterly manner with the names of Roman history and tradition. That is denied us in the present work, but certain conclusions based on the archæological data are put forward with due reserve. The most striking aspects of Dr. Randall-MacIver's position are as follows: (i) The Villanovans were not direct descendants of the Terramaricoli, but represent a later wave of kindred people from Central Europe; (ii) the early cremating people of Latium were Villanovans; (iii) the Etruscans only arrived in the eighth century when the Villanovan civilisation was already advanced; (iv) they did not bring the idea of the chamber tomb with them from the East. On the last two counts our author's thesis is at least the most convincing solution of the Etruscan problem that has yet been advanced.

His view of the Villanovans in effect adds yet another theory to the many answers put forward to the question—Who were the Romans? Of these, only three beside that before us need serious consideration. Pigorini, Colini and Peet have argued that the Terramaricoli (Italici) were the immediate ancestors of the Villanovans (Umbri) and the cremators of Alba and the Forum (Latini). Modestov agrees with Dr. Randall-MacIver in ascribing the Villanova culture to a fresh tide of immigrants from Central Europe, but follows the others in driving the authors of the Latian necropolis from the *terramare* folk. Finally, von Duhn (whose valuable work presumably appeared after the present book was in the press) follows Pigorini and his school with regard to the Italici, but regards the later inhumation graves found in Villanovan necropoles as marks of the advance of the Umbri. However, many of von Duhn's inhumations are on convincing grounds now attributed to Etruscans.

Personally I do not think the archæological evidence in Italy can alone decide between the three remaining theses. The choice must depend chiefly on the degree to which they illustrate traditional and linguistic data. Our author expressly refrains from attempting an interpretation in these terms. Still, it is tempting to venture on to the forbidden ground. The Villanovans, whose culture Dr. Randall-MacIver finds yielding ground to the Etruscan, must surely be the Umbri from whom the Etruscans took three hundred cities. But if the Forum graves, too, are Villanovan, where are the Latini or Q-Italici? On the other hand, the form of the Roman *castra* is in itself sufficient to prove a *terramare* element at Rome. As a matter of fact, the "Villanovanism" of the Alban cemeteries and the Forum is not to me obvious. Este I. is quite as distinctively Villanovan if, as we read on p. 97, the biconical ossuary is conclusive proof of "Villanovanism." Yet our author excludes not only Este II. (which is Venetic), but also Lozzo and Este I. from Villanova proper. Either Pigorini's or Modestov's theory, therefore, seems to fit both the material and the linguistic facts, while the new hypothesis is hard to reconcile with the latter. However, the hypothesis in question is only offered as a tentative suggestion. Our appreciation of the admirable manner in which the more important task of collecting and arranging the facts has been carried out is in no wise dependent on its acceptance.

V. G. C.

America, Central: Archæology.

Gann.

In an Unknown Land. By Thomas Gann. London, 1924. Pp. 256. Price 12s.

44

Dr. Gann is already well known as the writer of much valuable work on the Maya, and in the present book he gives an interesting account of a journey of investigation made, along with Dr. S. G. Morley, for the Carnegie Institution of Washington. The sites visited were in the little-known territory of Quintana Roo, as well as some of the well-known ruins in Yucatan. The account of the former region brings out clearly the distinct character of the remains there, which are, no

doubt correctly, attributed by the author to a late and decadent Maya culture having its centre at Tulum. It is noteworthy at the same time that Tulum should have a stela with an initial series, a fact which indicates that it was occupied by the Maya at an early period, and that, after having been abandoned for a long time, it was reoccupied by the later inhabitants. A parallel case is that of Flores, the last stronghold of the later Maya. This site is known to have been only settled by the Itzas after the fall of Mayapan, yet it has inscriptions of the Old Empire.

The full scientific results of this expedition will no doubt appear elsewhere, but a useful summary of the dates deciphered by Dr. Morley is given, although the date of the Temple of the Two Lintels at Chichen Itza should be transcribed as 11-7-12-16-18 instead of 13-7-12-16-18. This must be merely an oversight, but it is surprising to see in the summary of the calendar the statement that calendar round and period ending dates were peculiar to the New Empire. Both, on the contrary, are best exemplified in the inscriptions of the Great Period of the Old Empire, and occur even earlier, while in the New Empire the system of dating took on rather anomalous forms and was finally superseded by the Katun Count.

The book is extremely valuable in the information it gives as to both the ancient Maya and their living descendants. It is interesting to compare the account of present-day conditions in Yucatan with that of Stephens—who, like Dr. Gann, wrote a book both of scientific and general interest. The author has a strong sense of humour and writes in an amusing and interesting style. One gets the general impression that Yucatan is becoming like countries much nearer home, and not alone in the matter of Ford cars and high-heeled shoes. There are a large number of illustrations which add to the value of the book.

RICHARD C. E. LONG.

Technology : Smoking-Pipes.

Dunhill.

The Pipe Book. By Alfred Dunhill. London : A. and C. Black, Ltd. 1924. Pp. 262. Price 18s. net.

45

In his foreword the author proclaims that this is "no learned Treatise, but a simple Book." Nevertheless, he has clearly devoted much time and energy to the riding of his hobby, and has taken pains to describe and figure representative pipes from all over the world. He need not fear the onslaught of angry ethnologists, armed with "quivering fountain pens," since he has done little to stir their wrath. He has, on the contrary, shown a tendency to accuracy, and has, in the main, avoided the dangers of facile generalisation. That ethnology is not his subject is evident at times, but this does not seriously detract from the value of the work. The numerous sketches and photographs (in 28 plates and 230 text-figures) will help greatly to render it of service to those who have charge of museum collections in which smoking pipes are represented.

Mr. Dunhill is evidently not only an enthusiastic pipe-collector—many of the illustrations and descriptions are from his own collection—but a confirmed smoker. He is, in fact, an ardent Tobacconist, to use that word in the sense in which, as he points out, it was sometimes used in the seventeenth century. Nevertheless, we may doubt whether tobacco-smoking is as harmless and sustaining as he would have it, and as some of his authorities argue; whether it is, in fact, much more than one of those pleasant vices which have so far escaped the prohibitionist. But if all habits are bad habits, some are worse than others, and smoking may pass without too much discredit.

The study of smoking-pipes has suffered some neglect in consequence of the fact that they are, in the greater part of the world, of relatively recent origin, and

were spread in the first instance by civilised man. Whether in the Old World the habit of smoking arose entirely through the transference of tobacco and the accessories from the New World in the sixteenth century, is a point on which different opinions may be held, though the evidence for any other conclusion is slight. It is clear, however, that the form of most modern tobacco pipes can be traced to introduction, usually followed by modification due to local circumstances. Of such transfers the author has much that is interesting to say.

In the arrangement of the book, "Why men smoke" is followed by accounts and figures of makeshift pipes, tube pipes, mound pipes, American Indian pipes and smoking customs, pipes of the Far North and the Far East, water-pipes, African pipes, clay and other European pipes, and the modern briar. There are excursions in various directions throughout the book, and the "human interest" is always kept in view.

The illustrations are clear, and form an adequate basis for identification of specimens; the plates are excellent, and include four in colours. H. S. H.

Australia: Ethnography.

Horne: Aiston.

Savage Life in Central Australia. By G. Horne, V.D., M.A., M.D., Ch.B., and G. Aiston. London: Macmillan & Co., Ltd. 1924. Pp. 184. 18s. net. 46

This book deals mainly with the Wonkonguru, who occupy a region to the east of Lake Eyre, and it is based partly on the observations of Mr. Aiston, who lived amongst the aborigines for over 20 years, and partly on the results of a visit to the region by Dr. Horne. The latter is the responsible author, and if he has not marshalled his material in the sedate and orderly manner to which we are accustomed—the book is rather of the nature of a spate of ethnographical and other notes, held up at intervals by a chapter heading—he has given us much information of value, in a form that is interesting. Perhaps we should like to have heard a little more of the "very fat Cooriebakupontatagunta" who got up to join in a dance, without aid from her arms; and one wonders what Billy Welsbey did for teeth, after the crow stole his false set at Ooroowillanie; but Dr. Horne has more important things to tell us, and his splashes of local colour are not inartistic.

Although there are brief accounts of myths and magic, death and burial, rites and ceremonies, a substantial part of the book deals with technology, especially in relation to the manufacture and uses of stone implements. Of the 90 half-tone illustrations—most of them very good indeed—a large proportion illustrates aboriginal arts and crafts.

The interest taken by Sir Baldwin Spencer in the production of the book is a valuable independent testimony to the importance of the observations made by the two authors, and we have to thank them for placing on record another addition to our detailed knowledge of native tribes whose culture is for the most part already in decay. Civilising influences are varied and attractive, and when a black, at the sight of a white, dons in haste a shirt or trousers, lest a whip should chasten his nakedness, they are clearly irresistible. H. S. H.

CORRESPONDENCE.

Britain: Archæology.

Burkitt.

The Solutrean Culture in Britain.

To the Editor of MAN.

47

SIR,—This reply to Mr. Moir (MAN, 1925, 11) is in no sense written in a controversial spirit. My conception of the function of MAN is that it provides a place where

anthropologists can make suggestions or question the suggestions of others, and I am only answering now because Mr. Moir has questioned a point in my "Archæological Notes" for January not there elaborated. Personally, I confess I am of opinion that a laurel-leaf is not by itself typical of the Solutrean culture. As a matter of fact, I rather doubt if there is any type that by itself as an isolated specimen is a *certain* "landmark": it is the concatenation of proofs—typology, associated fauna, stratigraphy, etc.—that demonstrates the probable or absolutely certain presence of a given culture. But I admit I feel even less certain of laurel-leaves as type "fossils"—especially when found singly—than of more specialised types, such as split-base bone points or beaked burins or single shouldered points with thinning flaking on the under surface at the point, because a similar technique occurs in the case of the large leaf-shaped javelin points found by Mortimer with early metal objects, and these grade insensibly into what are typical laurel-leaves. Moreover, the laurel-leaf type—a natural shape for a javelin head when all is said and done—is found in many areas to the South, outside Europe, where it is difficult to imagine the Solutrean culture—essentially Northern as it is, and not found in Central or South Spain, in Italy or in Palestine. Such an area is the oasis of Siwa, which provides plenty of laurel-leaves, grading insensibly into ordinary pre-Dynastic types of tools. As regards the writings of other authors, I feel it must be remembered that Breuil's paper on the subdivisions of the Upper Palæolithic periods was written as long ago as 1912, and that both he and Obermaier in his book, are only really dealing with Palæolithic industries. Certainly I would agree with Mr. Moir that where the laurel-leaf occurs in a Palæolithic *milieu*, with Quaternary fauna, the presence of a Solutrean culture is certain. Where, however, the finds are isolated, in my humble opinion it is dangerous to be dogmatic. In the case of the isolated find of a shouldered point from Lakenheath (a type which, as I have said, appears to me to be more specialised and useful as a "fossil" than the laurel-leaf) it should be noted that I only suggest the possibility of Solutrean influences in the district. Even admitting that Mr. Moir's four or five finds of isolated specimens of laurel-leaves are Solutrean in age, it would still seem to me bold to introduce therefrom a fresh race into East Anglia. It is a different story at Kent's Cavern, where the laurel-leaf type occurs in an intermediate layer of the Upper Palæolithic series.

As to why the remains of "cave man" are apparently rare in East Anglia, surely it is largely a question of climate? I have always imagined that the reason for the scarcity in open stations was the unsuitability of the climate at that epoch in our latitudes. The Wurmian ice sheets remained long in the Scandinavian mountains and possibly these still further militated against East Anglia being a salubrious hunting-ground and open-air residence.

In conclusion, I should like to thank Mr. Moir for bringing this matter prominently forward and it will be interesting to have other people's ideas. My theses are: that it is dangerous to argue much from isolated finds of single specimens; that if a tentative suggestion is deduced from an isolated find, more specialised tools, such as the single-shouldered point with thinning flaking on the under surface at the point, etc., are better guides than the more ordinary types, including here laurel-leaves; but that in any case typology *alone* is always an unsafe guide.

M. C. BURKITT.

P.S.—Alas! I have never visited Volgu. But the original publication quoted dates back to 1874, and I have sometimes thought, when in a bold mood, that it might be worth the trouble to reinvestigate the origin and age of this anomalous find of incredible tools.

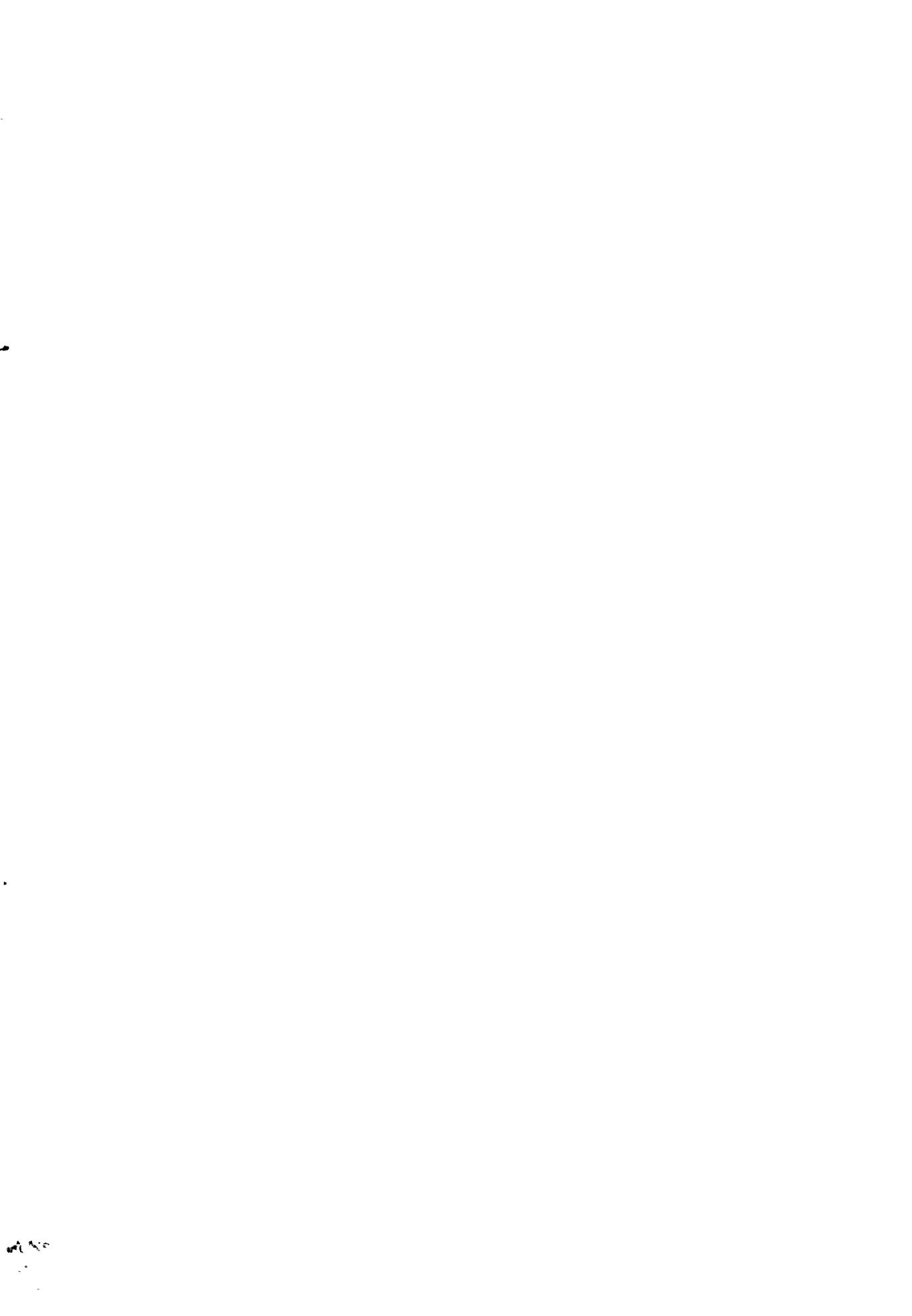




FIG. 2.



FIG. 1.
ARCHAIC SCULPTURED ROCKS OF GORGONA ISLAND, SOUTH AMERICA.



FIG. 3.

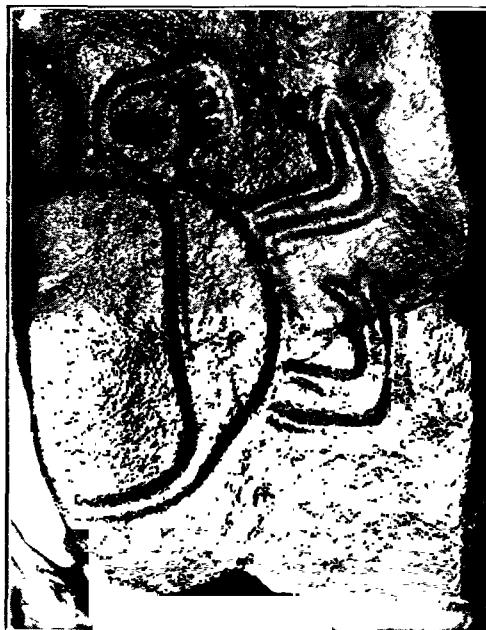


FIG. 4.

ORIGINAL ARTICLES.

With Plate F.

America, South: Archaeology.

Hornell.

The Archaic Sculptured Rocks and Stone Implements of Gorgona Island, South America. By James Hornell, F.R.A.I., F.L.S., Ethnologist to the "St. George" Expedition. **48**

During a visit (3rd to 12th July, 1924) of the "St. George" Expedition* to the island of Gorgona, 17 miles off the coast of Colombia, an important discovery of sculptured stones and neolithic implements was made by the writer. These were found on the shore of Watering Bay, on the east coast of the island. The sculptured rocks consist of (a) a large number of boulders whereon are deeply incised designs of archaic character—these lie well below what is now high tide level—and (b) a set of four bearing sculptures of comparatively recent origin, lying at a higher horizon, where they are uncovered by the tide for the greater part of the day.

At the present time the sea is encroaching on the land in the locality where these stones were found. The butts of several old coconut trees are still to be seen *in situ* a considerable distance within tidal limits, and on the sand-spit itself several neolithic stone implements, *little water-worn*, were picked up on the surface. Further, the majority of the older sculptures are on rocks close to the low tide level of spring tides, where it would be difficult under present conditions to execute them, as they are uncovered for a short time only each tide.

The boulders bearing archaic sculpturing formed two groups, a small series near the middle of the bay and a much more numerous one at the south end; the former lay generally at about half tide level, the latter just above low water. In all cases the boulders chosen for sculpturing are of picrite, a dense, fine-grained, very dark igneous rock, of a rather greenish tint where weathered.

The smaller series consisted of four rocks bearing sculptured designs, comparatively well preserved, and a number of others whereon it was just possible to certify that designs had been incised. The four principal rocks of this series were by far the most important of those found, their designs being the most definite as well as the best preserved. They apparently had formed an ordered group centering round a huge roughly quadrangular boulder bearing on its upper surface the representation of a pair of rude ungainly human figures, male and female, each with a number of rays disposed around the head, forming a halo (Pl. F, Fig. 1). These figures stand side by side, the man on the left, the woman on the right as one looks at the stone. They are graved upon a great conchoidal fracture which measures roughly 2 feet high by 2 feet 10 inches wide: it is almost entirely filled by the two figures.

The male figure (Fig. 1) measures 1 foot 10 inches in height, inclusive of the head rays: the width is 1 foot 3½ inches. The head is subtriangular, the features

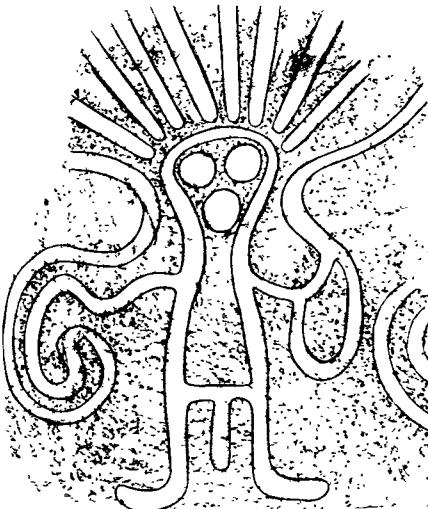


FIG. 1.—THE MALE FIGURE ON THE "SUN-GOD" STONE, PHOTOGRAPHED FROM AN OUTLINED RUBBING.

* Organised by the Scientific Expeditionary Research Association and under the direction of Commander D. Blair, O.B.E., R.N.R.

formed of three cup-shaped depressions, two above for the eyes and one, slightly larger, below, to represent nose and mouth. The lower part of the face passes into the trunk, through a slight constriction which may represent the neck. The body is short and stout, widening considerably in the abdominal region. From each lower corner of the wide trunk a straight short leg is given off, represented by a single broad, round bottomed channel, and terminated by a large out-turned shapeless foot. A straight penis of disproportionately large size hangs pendant midway between the legs. The arms are asymmetric and are the most peculiar feature of the figure. The right passes outwards from the shoulder, then curves downwards and, after making a whorl of two sinuous twists, turns upwards to the level of the shoulder, whence it curves in to the side of the head, to merge finally into the outermost ray on the right side thereof. The left arm follows a less involved course: after leaving the shoulder it forms a simple loop and then turns upwards to join the outer ray on the left of the head in the same way as its fellow on the right. The rays themselves are straight grooves radiating from the upper part of the head, ten in number, inclusive of those into which the arms merge.

The female figure (Fig. 2) closely resembles the male, except that the body is narrower, the lower limit of the abdomen not shown (possibly this signifies the use of a skirt or cloth wrapped around the body and legs), and the rays around the head, 13 in number, instead of 10. No mammae are shown. The puzzling sinuosities and also the striking differences between the right and the left arm in the case of the male figure, are reproduced with exactitude, and leave no room to doubt that these were premeditated and had definite meaning for those who carved the figures.

The outlines here, as is the case with all the archaic sculptures yet to be described, are formed by broad, shallow, rounded grooves about $\frac{3}{4}$ to 1 inch in width, the edges usually considerably degraded by weathering; depth about $\frac{1}{4}$ inch or slightly more.

FIG. 2.—THE FEMALE FIGURE ON THE “SUN-GOD” STONE, PHOTOGRAPHED FROM AN OUTLINED RUBBING.

Forty-eight feet S. 8 degrees W. of this central stone was another large one, sub-cuneate in shape, the largest and smoothest face inclined at an angle of about 40 degrees and oriented to the east. On this is rudely sculptured a stepped pyramid formed of four superimposed platforms, progressively decreasing in size upwards. The basal platform, or storey, is low, with a very wide step, the next is considerably higher, while the third and fourth are about equal in height to the basal one. The apical or fourth storey is very narrow, the height half again as great as the width. A bold gutter-shaped ray diverges from the outer angle of each platform and another from each of the inner angles of the two lower ones. Six circular depressions or cups occupy the face of the third storey and the upper half of the second. Below the straight groove which forms the base line of the pyramid and running into it, are two semi-circular gutters; the outer with a diameter equal to the width of the second storey. The ends of the inner one are continued inwards upon the face of the pyramid, the arm on the left passing within that of the right, giving the beginning of a coil. (Pl. F, Fig. 2.)

This stepped pyramid may possibly represent an early form of the pyramidal temples of Mayan and Aztec culture; this similarity and the presence of rays

suggests that the figure was intended to depict a temple of the Sun, such as that to Tonatiuh, at Teotihuacan in Mexico, while the six cups may represent astral divinities. If this interpretation be correct, then the two anthropomorphic figures on the central stone may well be those of Tonatiuh himself and of his wife (or sister) Metztli, the Moon. The curved grooves are more difficult to interpret; possibly the outer one was meant to represent the walled enclosure surrounding the temple pyramid, while that within, with its short coil, suggests the sacred snake, intimately associated with Sun worship. Similarly it furnishes evidence of Mexican culture having extended southward to the coast region of Colombia. This would be in agreement with the fact that the Indian tribes of the Pacific slope districts of Colombia (Antioquia, Cauca, etc.) preserve characteristics more akin to those of the Aztecs than to any other race.*

The other two boulders of this group are both smaller and on each the sculptured design is that of a monkey. The first (Pl. F, Fig. 3) lies 42 feet north of the central stone; the second (Pl. F, Fig. 4), 15 feet east of it. The former is a stone three feet eight inches in length, roughly triangular in cross-section. As it lay on one face, it presented a ridge-like shape, the long axis north and south. On the sloping seaward face (1 foot 5 inches deep), oriented to the east, is the rudely outlined figure of a monkey, with a stiff upstanding tail arched over the back. The design is crude and even childish in conception, and without either vigour or artistic feeling, as will be seen on reference to Pl. F, Fig. 3. The head in particular is weak in design. Two cup hollows, vertically disposed near the centre, are probably intended to represent the ear; the face looks downwards, the features represented by two deep "bays," one for the eye, the other for the mouth. The animal stands on all fours and it is difficult to be certain whether the artist intended to depict all four or two only. If, as I believe, four legs are present, then two toes only have been given to each. On the landward (west) side of the same stone, is a graving of a second monkey, executed in a similar attitude and design, but rather smaller (1 foot 4 inches in length).

The second monkey stone, lying 15 feet east of the central "god" stone, is a large wedge-shaped boulder that lay on one of its two tabular faces; the other large face was exposed, with its inclination to the east. This surface measures roughly three feet in length by the same in width. On this is deeply and boldly graven the image of a large monkey (Pl. F, Fig. 4), far more realistic in execution than the smaller ones on the other stone. It measures 1 foot 10 inches in length by an overall height, inclusive of the tail, of 2 feet 2 inches. The pose is one of alertness and very different from the downcast, lifeless attitude of the other two monkey figures; the head is held well up and the face in consequence looks forwards, instead of downwards. The tail, in place of being stiffly arched over the back, is bent downwards at half length, almost forming a loop; at the point where the tip comes to rest upon the creature's back it passes into a terminal device that appears to be a bird's neck and head, the beak open so widely that the upper mandible forms a right angle with the lower. The former is strongly curved towards the distal end and, if we accept the device as representing a bird's head, it is probably intended for that of a flamingo. The absence of a tongue, apart from the form of the mandibles, rules out any idea that it is meant for the head of a snake.

As in the other monkey figures the legs, of which all four are shown, are formed by a single deep groove. The legs of the right side (the animal faces to the right) are shown behind those of the left. Each of the former seems to be provided with three toes—they are somewhat indistinctly graved. The termination of each of the left legs is not shown, the distal part of the left fore leg running forwards and upwards

* *Encyclopædia Britannica*. Eleventh Edition, Article, "Colombia."

to the margin of the stone, whilst the similar portion of the left hinder leg runs in a like manner as a deep wide groove upwards and forwards till it merges with the groove outlining the abdomen. The legs in all the three figures are flexed at right angles at the elbow and knee respectively.

On the flank is a solitary shallow "cup" depression and another occurs towards the bottom left-hand corner of the stone.

The graving is particularly bold, the grooves being from $1\frac{1}{4}$ to $1\frac{1}{2}$ inches wide, by fully $\frac{1}{4}$ inch in depth.

(*To be continued.*)

Britain: Archaeology.

Garrod.

Solutrean Implements in England. By Miss D. A. E. Garrod.

49

The notes contributed by Mr. Burkitt (MAN, 1925, 3, 47), and Mr. Reid Moir (MAN, 1925, 11), having raised once more the question how far England was influenced by the Solutrean culture, it is, perhaps, worth while considering what really is the present state of our knowledge on this subject.

Mr. Burkitt speaks somewhat vaguely of the "rare examples of Solutrean types found in England (Kent's Cavern, etc.)." As a matter of fact the implements to which he refers are not truly Solutrean, but are proto-Solutrean forms such as are found on the Continent in Upper Aurignacian levels of the Font-Robert type. Nor

are they rare in this country; on the contrary they form a large proportion of the total number of implements found, so far, in the English caves, and the following list of sites in which they occur will show how widely they are distributed:—Kent's Cavern, Bench Cavern (Brixham), Wookey Hole Hyæna Den and Uphill Cave (Mendips), Paviland (Glamorganshire), Ffynnon Beuno (N. Wales), Cresswell Crags (Derbyshire).

The leading form of this industry is a blade showing pressure flaking on the bulbar surface only, either at the tip or at the tip and base, and differing in no respect from similar implements found in Font-Robert levels abroad (Fig. 1, Kent's Cavern). In some of the English specimens, however, the flaking extends over the whole of the bulbar surface (Fig. 2, Cresswell Crags), and in others there is a certain amount of flaking on the upper surface as well, while, occasionally, bifacial ovoid implements, forerunners of the true laurel-leaf, are found associated with these forms (Fig. 3, Wookey Hole Hyæna Den). The rather special development and wide distribution of this industry in England suggests that it replaces, to some extent at least, the true Solutrean, but it is worth noting that at Cresswell Crags two fragments of typical laurel-leaves were found (Fig. 4).

With regard to implements found on the surface or without accompanying fauna, I agree with Mr. Burkitt that the laurel-leaf is not necessarily a typical tool; but certain specimens found on the surface or in isolated deposits in East Anglia differ so widely in formation from the typical Neolithic of the district, and so

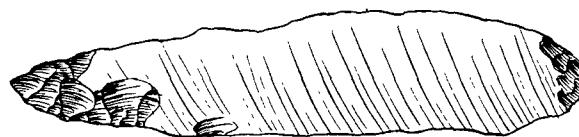


FIG. 1.—KENT'S CAVERN.

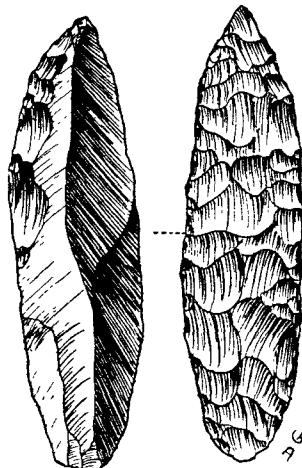


FIG. 2.—CRESSWELL CRAGS.

closely resemble Solutrean implements in form and technique, that Abbé Breuil, who has recently examined some of them, is of the opinion that they are almost entirely Solutrean.

Mr. Burkitt's contention that the Upper Solutrean shouldered point is a more satisfactory "fossil" is certainly true; but his statement that the specimen from Lakenheath figured in his article, and the French specimen with which he compares it, are typical of this form is inexact. The French implement may come from an Upper Solutrean level (he does not give its *provenance*), but in that case it is an unfinished specimen, since the typical shouldered point is a far more perfect object, flaked over the whole of the upper surface. Typologically the point figured by Mr. Burkitt, with localised retouch defining the shoulder, and

pressure-flaking on the bulbar surface only, is far more like the "*pointe à cran atypique*" of the Upper Aurignacian, and its resemblance to the Lakenheath specimen is, therefore, valueless as a basis for the suggestion that the Upper Solutrean is present in England.

To sum up, we find in this country a high-developed proto-Solutrean level of Font-Robert type, indications at Cresswell Crags and Ipswich of a typical Solutrean

influence, and, so far, no trace whatever of the Upper Solutrean of Central France.

D. A. E. GARROD.

Totemism.

Róheim.

Cú-chulainn and the Origin of Totemism. By G. Róheim.

50

It has been pointed out by various authors that some of the Irish *geasa* and notably the dog taboo of Cú-chulainn are probably totemic in origin.* We fully endorse this view and intend to go even further: we believe that the saga of Cú-chulainn throws some light on the much disputed question of totemic origins.

There is nothing particularly remarkable in the idea that a hero who is frequently called Cu, *i.e.*, dog, should be prohibited from eating dogs' flesh,† and it is also quite natural that the broken taboo proves fatal to the hero of the dog clan. What calls for attention is that Cú-chulainn only becomes a dog by killing a dog,‡ much as the manitous or personal totem must be killed in certain North American tribes before it endows mortal man with the faculties belonging to the supernatural animal. Now Freud regards totemism as the form of social organisation that arose out of the repression of the great conflict between fathers and sons for the women in the Cyclopean family: in other words, as co-eval with exogamy, and

* A. Lane, "Custom and Myth," 1904. 265. G. L. Gomme, "Folk-Lore as an Historical Science," 1908. 286, 287.

† E. Hull, "Old Irish Tabus or Geasa." *Folk-Lore*, XII, 49, 50. Cf. Windisch, "Tain bo Cualnge," 1905. 444, 888.

‡ E. Windisch, "Tain bo Cualnge," 1905. 118.

§ Cf. for the explanation of manit *i*-killing, Róheim, "Das Selbst": *Imago*, VII, 492.

|| S. Freud, "Totem and Taboo," 1919. Lang-Atkinson, "Social Origins and Prima Law," 1909.

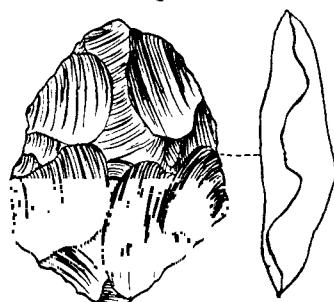


FIG. 3.—WOOKEY HOLE HYÆNA DEN.

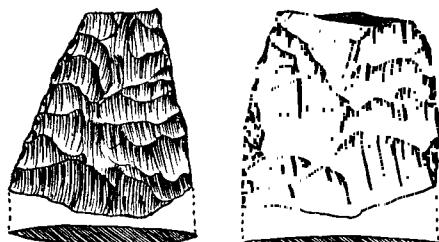


FIG. 4.—CRESSWELL CRAGS.

in the same sense as the latter, an embodiment of the Primal Law. Totemism is a system of defence against the possibility of relapsing into the two crimes of Oedipous: man is not allowed to kill the totem-animal, whom primitive tribes regard as their father, and to mate with women belonging to the same totem, *i.e.*, to commit incest with the mother.* According to this view, totemism arises after the violent death of the Jealous Sire out of the feeling of guilt and compunction in the young males of the horde who had done the deed to which they were prompted by their genital impulse. I think it will go far to confirm this theory if we can show that the totemic saga of Cú-chulainn contains more or less veiled traces of the conflict between father and son, of the animal symbol or name originating out of a feeling of guilt for the act of parricide, and of incest committed by the hero.

One of the youthful feats of our hero is to slay the supernatural watch-dog of the smith Cu-lann. He offers to serve in the dog's stead, guarding the smith's stronghold and cattle till he finds another dog of the same breed.† It is a typical feature in the life of Aryan heroes that they serve a term of apprenticeship with a smith from whom they usually obtain their terrific weapon.

Gratitude is not one of the outstanding virtues of our heroes, for they frequently kill their benefactor.‡ Now primitive people usually regard smiths as uncanny or supernatural§ and it is very probable that this quality is derived from the awe felt by the child before the mystery of its own origin, the life-giving sexual act.¶ For primitive mankind all objects are endowed with life and therefore to make a tool is the same thing as "making," *i.e.*, procreating a new human being.¶ This is how the smith becomes a representative of the father, and we find that Tvashtri the smith, killed by Indra, is really the father of the hero-god.** In some variants of the Bear's-Son type we find the hero killing a smith, whilst in others he is represented as another Oedipous who deals his father the mortal blow.†† We shall, therefore, conclude that, mythologically speaking, the smith and his dog are but "Abspaltungen" of the same unconscious idea,†† and that Cú-chulainn derives his totemic name from the very act of parricide which it serves to hide. The hero becomes a dog when he has killed his father who was a dog. §§ His servitude as the smith's watchdog is not merely the practical settling of a question of property, but, strictly analogous to the term of penance undergone by Greek

* Freud, *l.c.* 239.

† Windisch: "Tain," 118.

‡ Cf. Siegfried and Mime; Grimm, "Deutsche Mythologie," 1875. 315. F. Panzer, "Studien zur germanischen Sagengeschichte II." Siegfried, 1912. 42. In Rome we have the annual expulsion of Mamurius Veturius, the smith-father of the hero-god Mars, by the Salii, who represent the young god himself. L. Preller, "Römische Mythologie," 1838. 296. The Finnish Kullervo, who commits incest with his sister, serves as a slave of the smith Ilmarinen and kills his master's wife. "Kalevala," *Runo*, XXXI-XXXVI.

§ R. Andree: "Ethnographische Parallelen und Vergleiche," 1878. 153. O. Schrader, "Sprachvergleichung und Urgeschichte," 1906, II, 13. For the type of folk-tale in which the uncanny aspect of the smith survives, see Macdougall and Calder, "Folk-Tales and Fairy Lore in Gaelic and English," 1910, 17.

|| Cf. B. Gutmann, "Der Schmied und seine Kunst," *Z. f. E.*, 1912. 83, 87. The Dsagga regard the power which unites blade and shaft, *i.e.* man and woman, as supernatural.

¶ In Egypt the sculptor is "he who causes to live" and to "fashion" is "to give birth." G. Elliot Smith, "The Evolution of the Dragon," 1919. 25.

** H. Oldenberg, "Die Religion des Veda," 1894. 234, 235.

†† F. Panzer, "Studien zur germanischen Sagengeschichte," 1910. ✓23, 39, 52.

†† Cf. A. Nutt, "Aryan Expulsion and Return Formula." *Folk-Lore*, IV, 26. Originally it was the smith who was killed by the hero.

§§ Cf. a Transylvanian legend on the struggle between father and son, who both appear as bears. Hermann, "A hegyek kultusza." "Mountain cults." *Erdély*, 1893. 182.

dragonslayers after their heroic, yet murderous deed.* It is an act of expiation, of "subsequent obedience," if Cú-chulainn becomes a dog after having killed a dog.†

Anthropologists will have noticed that the Law of the Pack represented by the hypothetical primal state of human society re-appears in the case of the King of Némi who is king as long as he holds his own against the new pretenders to his office. Originally, danger must have threatened the leader of the herd from his own sons, who would be the natural successors to the throne. Some slight trace of the original pre-human state of things perhaps survives in the figure of Conchobhar, Cú-chulainn's uncle, who—although the mightiest warrior on earth—was not suffered to encounter any danger "for the preservation of the king's son,"‡ a curious taboo which becomes comprehensible if we regard his son as Conchobhar's adversary, whom he must inevitably have killed in the combat. In an ingenious paper on the "European Sky God," A. B. Cook shows that some of the taboos which prove so fatal to the "fortissimus heros Scottorum" are really those of the divine king of Tara.§

Since the publication of the "Golden Bough" no anthropologist doubts the specific importance of the oak for Aryan divine kings; Cú-chulainn is bred in an oak-house,|| he kills Roi, Son of the Oak, and mates with Curois' wife Blathnath.¶ This is the natural thing for a King of the Wood of the Némi type, and our interpretation of the taboo laid upon Conchobhar finds a striking confirmation in the fact, that what is feared in him, actually comes to pass in the person of his nephew: the invincible Cú-chulainn fights and kills his own son.** There seems reason to suppose that this is not the only case of the father-son combat in the Cú-chulainn cycle, for Lugaid, the man who deals him the death-blow, is the son of Curoi of the Oak, with whose wife Cú-chulainn had intercourse. This is nearly as much as to say that he is Cú-chulainn's own son, and thus it would be quite comprehensible that Cú-chulainn should part with a woman who ought to belong to him and pass her on to Lugaid.†† The unconscious content of the episode can only be interpreted from the infantile Edipous-wish: Cú-chulainn handing the princess on to Lugaid, is the father relinquishing his claim to the love of the mother in favour of the son.†† We guess that the amorous relations of our hero to Blathnath, the wife of Curoi, correspond to the same situation; in other words, Curoi the Oak-man whom he kills, is really the father of Cú-chulainn. For is not the uncouth giant Curoi simply an archaic form of the divine king? Now, Conchobhar of the

* Apollo: Apollodor. III, 3, 2. Kadmos: Schol., II. II. 494; Ov. Met. IV, 563. Like Cú-chulainn Kadmos is transformed into the animal he has killed. For the totemistic origin of the myth, see Frazer, "Dying god," 1911, 70, 84.

† Cf. Freud, "Totem and Taboo," 238.

‡ Book of Leinster, fac. pp. 106, a, 33–107b, 16, quoted by E. Hull, "Old Irish Tabus." *Folk-Lore*, XII, 54.

§ A. B. Cook, *Folk-Lore*, XVII, 1906. 336.

|| J. Pokorny, "Der Ursprung der Arthursage," *Mitt. d. Anth. Ges.*, XXXIX, 1909. 113.

¶ A. B. Cook, *l.c.*, 335, 336.

** M. A. Potter, Sohrab and Rustem. *Grimm Library*, XIV, 1902. 22–28.

†† A. Nutt, "Cú-chulainn, the Irish Achilles." *Popular Studies*, No. 8, 1.

†† Cú-chulainn sucks the blood of the woman he has rescued and therefore cannot marry her. Modern parallels show the same ritual act with the opposite result: as a betrothal or marriage ceremony. (F. C. Conybeare, "A Britanny Marriage Custom." *Folk-Lore*, 1907. 448.) In both cases the explanation is to be sought in the fundamental, though ambivalent, nature of the mother-son relation for love-life; the woman from whom the hero sucks blood (a substitute of milk) represents the mother. The Swanetian knight sprinkles the breast of his lady with salt, and then touches it with his teeth, repeating thrice: "Thou mother, I son." (Singer, "Blood-Kingship." *Folk-Lore*, XIX, 1908. 344.) The hero who obtains her hand may perhaps have been regarded as a reincarnation of Cú-chulainn's father Lug (Lugaid derived from Lug; A. B. Cook, "The European Sky-God." *Folk-Lore*, XVII, 343) and hence the rightful husband of Cú-chulainn's mother.

Ultonians is originally not only the uncle, but also the father of Cú-chulainn, for the divine fatherhood of Lug is only a thin mythical veil to cover the incest of Conchobhar with his sister Dechtire.* Born of incest, Cú-chulainn must commit incest, and if we openly find him engaged in mortal combat with his son, we must conclude that this is but the repetition of another battle fought between him and his father.†

If we have been right in equating the smith with his hound it will be significant to find that Curoi mac Daire is not only an Oak man, but also the Hound of Roi, Son of the Oak, and that the battle is, therefore, between two heroes of the dog clan.‡ Finally, when Cú-chulainn breaks the dog-taboo and eats his totem animal, he is killed by Lugaid, who is, or might be, his son, at the same time bearing a name suspiciously like that of his father, Lug.§ The case of the Baja king who is compelled to eat his own totem animal when death at the hand of his son and heir draws near, is a striking parallel that illustrates the final conclusion of our speculations.||

We believe that man's primal conflict, the fight of successive generations for the women of the horde, is condensed in the life-history of the famous Ultonian hero, both in its totemic phase (men of the dog totem breaking through the cardinal taboos of totemism) and in the violent succession of the divine king to his father's throne.

G. RÓHEIM.

Africa, East: Archæology.

Dewey; Hobley.

Some Obsidian Implements from Kenya Colony. By Henry Dewey, 51
F.G.S., and C. W. Hobley, C.M.G., F.R.A.I.

Implements made of obsidian have been long known from Kenya Colony. The first examples recorded were collected by Prof. J. W. Gregory from the Rift Valley, and were described and figured in "The Great Rift Valley" (Murray, 1896).

* Cú-chulainn is the son of his maternal uncle Conchobhar, who does violence to his sister Dechtire. At the same time he has a supernatural father in the sun-god Lug, or rather he is Lug himself, *i.e.*, his own father (Nutt: Meyer, "The Voyage of Bran." Grimm's Library VI., Vol. II, 1897, 38-42. For a parallel case see *ibid.*, 26, Mongan; cf. Pokorny, I, c. 104). I intend to show, in connection with primitive views on the supernatural origin of children, that the myth arises as a substitute for the repressed incest-wish and both Cú-chulainn and his father Conchobhar are represented as reincarnations of the sun-god Lug. Was it the regular thing for the divine king to be the son of a brother and sister and re-incarnation of the sun? A striking analogy would be the case of the Pharaoh. Cf. A. Moret, "Du Caractère religieux de la Royauté pharaonique," *Annales du Musée Guimet*, XV, 1902, 38.

† From a psychoanalytic point of view it is significant that when the smith asks Conchobhar whether he may let the watch-dog loose, the king quite *forgets* Cú-chulainn and answers that all his followers are assembled, thus exposing the young boy to mortal danger (Windisch, "Tain," 122. Cf. on forgetting, Freud, "The Psychopathology of Everyday-Life").

‡ Cf. J. A. Macculloch, "The Religion of the Ancient Celts," 1911, 219, following J. Rhys, "Celtic Britain," 1908, 267. He is also called "three dogs" and his son Lugaid, "son of three dogs" (mac tri con, "Annals of Tigernach"), is another divine king and hero of the dog-clan. In an important variant of the dog-myth the hound of Culan is a reincarnation of Conganchness, who is the brother of Cu-roi, the Hound of Roi. K. Meyer, "The Death Tales of Ulster Heroes," Royal Irish Academy, Todd Lecture Series, Vol. XIV, 1906. Quoted from Nutt's review in *Folk-Lore*, XVIII, 230. I am compelled to content myself with secondhand references as most of the original sources are completely inaccessible here. Budapest (1, VIII, 1924).

§ He, in his turn, is killed by Conall, a hero whose horse has a dog's head. J. Rhys, "Arthurian Legend," 222.

|| The totem animals are leopard, hyena, white cock; and the person who compells the king to break his "geasa" and die in consequence, is his own son. L. Frobenius, *Und Afrika sprach*, 1913, III, 181, 182. We have interpreted the smith (whose name our hero inherits after the feat which may be regarded as his totemic initiation) as a representative of the royal father and here Frobenius tells us that the smith is an object of extraordinary veneration and regarded as standing nearest to the king.

From time to time accounts of artefacts found in different parts of the country have been published in the *Journal* of the East Africa and Uganda Natural History Society (Longmans & Co.). Illustrations of various types have also been reproduced (*vide* Part V, p. 22; Part VI, p. 60; Part VIII, p. 145; Part XI, p. 189; Part XII, p. 265). These implements are easily divided into three groups:—

(a) A series of boldly-flaked bouchers and ovates of St. Acheul type and size. The specimens from Kenya which belong to this group come from the Rift Valley near where it is traversed by the Magadi railway. They are made from the phonolite of the Kapiti volcanic series. They closely resemble some of the implements obtained by H. W. Seton Kerr from the Eastern Egyptian desert and the Somali plateau. Certain roughly-worked implements collected by Dr. F. Oswald may also belong to this group, but they are smaller than the ones referred to above.

(b) A series of obsidian tools, mostly scrapers, but occasionally a delicately chipped leaf-shaped arrowhead is discovered; pygmy implements are also common in certain areas. These obsidian artefacts have a wide range. A specimen was found near Kisumu, which is about 200 miles from the nearest known obsidian outcrop. They are, however, most numerous in the Rift Valley and on the slopes of the Kikuyu country.

(c) Polished implements. A remarkable axe made of a close-grained basalt was found near Eldama Ravine (figured in *E. A. Nat. Hist. Journal*, Part VI, p. 30). It is comparable with one from Kahun in Egypt (*vide* "British Museum Guide to Stone Age," p. 110, Fig. 114). Stone rings and bowls have been dug up from time to time in Sotik near Nakuru and Naivasha. Circular bored stones similar to the South African *Kwe* or *Tikoe* have been found in Masailand and at Taita.

Worked stone spheres of quartz and metamorphic rock have been found in Masailand and at Tsavo.

The above-mentioned three groups are classified from their appearance solely, for, so far, no positive geological evidence has been obtained from Kenya Colony regarding comparative age. It may well be that groups (b) and (c) are contemporaneous, the differentiation being merely due to difference of material.

A number of specimens have recently been received from a well-known site near Njoro, which is on the Uganda railway, 464 miles from Mombasa, and on the western flank of the Rift Valley: its altitude is about 6,900 feet above sea level.

We are indebted to Mr. W. A. Tunstall of Njoro for the examples which are now described, and he has collected many specimens on his farm during the last ten to twelve years. The site is to the south of the railway line and a mile or so south-east of Njoro station. The implements are found both on the surface and up to a depth of 18 inches below it, those from below appear identical with those on the surface.

They are found at various other spots within a radius of 20 miles of Njoro, and at an altitude of about 8,000 feet on the summit of the Mau escarpment, which forms the western wall of the Rift Valley in this region.

There is a small stream running through the land, but there are no implements among the gravel in its bed.

About a foot below the level of the soil a deposit of water-worn pebbles occurs, this possibly marks the shore line of a Tertiary lake. There are many such in various parts of the Rift Valley of the former Lake Suess, south of Lake Naivasha, described by Gregory in "Great Rift Valley." The Rift Valley is bounded by successive step faults which are generally more distinct on the eastern side than on the west: in this locality the blurring of the fault scarps is believed to be partly

due to the subsequent deposit of fine ash from the great volcano called Menengai near Nakuru.

As regards the source of the obsidian, there is none in the neighbourhood, but considerable outcrops occur in some of the more recent lava flows to the west and south-west of Lake Naivasha, about 40 miles away to the south-east of Njoro.

Numerous artificial flakes have also been collected from Gilgil, mile 412, on the Uganda railway; a stone bowl was found at Naivasha, mile 394, and another at Nakuru, mile 427. Some of the finest obsidian implements have, however, been found in the neighbourhood of Kikuyu Station, about mile 345 on the railway.

C. W. HOBLEY.

The series of implements represented in this collection can be divided into four groups, to which the French terms for such forms may be applied: (1) *Lame grattoir*; (2) *grattoir tarte*; (3) *burin*; (4) *tranchant rabattu*.

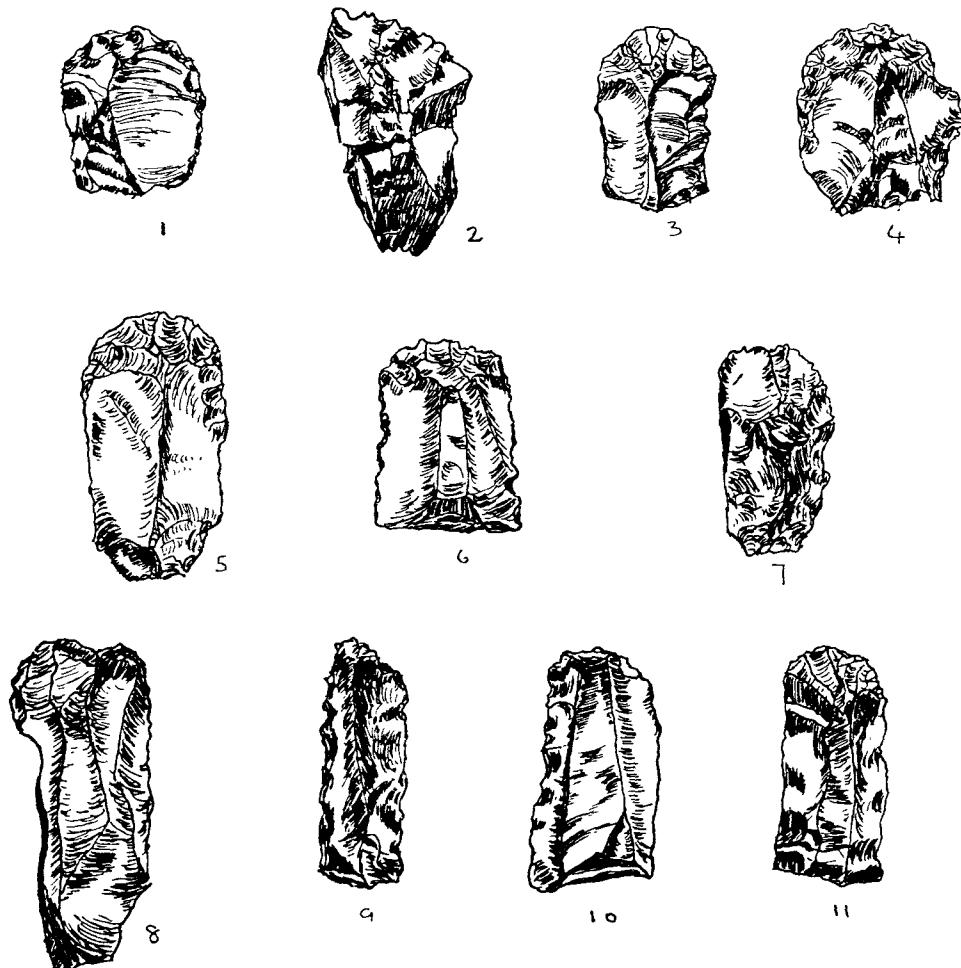


FIG. 1.

(F.E.H. del.)

The *lame grattoir* type is the commonest (Fig. 1). Most specimens have one or two ridges along the back and are beautifully retouched at the working end (Fig. 1, Nos. 5 and 11) (the length of the flake ranging from $\frac{3}{4}$ inch to 2 inches). In addition to this larger flaking there are numerous minute flake-scars around the periphery

of the pieces. Some specimens have been chipped as a result of use along one or both edges and at the end.

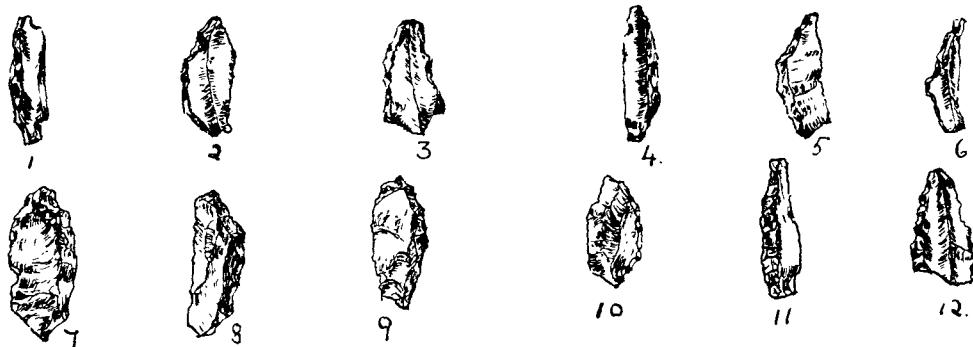


FIG. 2.

(F.E.H. del.)

One form is a handsome *grattoir* with symmetrical end-scarfs arranged in a fan shape, and shows a sort of nibbled edge (Fig. 1, No. 11). Typical forms are figured on Fig. 1.

The *grattoir tarté* and carinated planes (Fig. 1, No. 2) are closely comparable with the similar forms from the Dordogne Caves. Some might be termed nuclei, but the objection that readily occurs to the employment of this term is raised by the obvious results of the use of these pieces as scrapers or planes. Another one has bold symmetrical flakes struck off at the working end nearly at right angles to the long axis of the piece and resembles the carinated *grattoir* of Aurignac age from Corrèze, France, figured by Dr. Breuil.* (Fig. 1, No. 7.)

The specimens that possess the greatest interest are those that in their characteristics resemble the pygmy implements found in many parts of the world.

Among the smaller pieces there are many that are trimmed with care along the back by the removal of numerous minute flakes. The opposite edge is as sharp as a knife blade and, indeed, in shape these flakes resemble the blade of a pocket knife. Some are trimmed along the right edge (Fig. 2, Nos. 1, 2, 3 and 5) and others along the left (Fig. 2, Nos. 4, 7-11), the number of each being about equal. Dr. Breuil, in the paper cited, deals with the evolution of the Chatelperron Point and the Gravette knife and calls attention to similar forms from Tunis which have been assigned to the Capsian period, *i.e.*, an early Neolithic stage. These East African examples resemble closely (except that they are all minute in size) those that he figures on p. 170. As they are found in company with the larger specimens the industry cannot be claimed as a true pygmy industry, but it appears to be related. The pygmy implements are assigned by some French and Belgian archæologists to the Tardenoisian or an early stage of the Neolithic period, but others recognise in the style of workmanship traces of the Magdalenian influence. Pygmy implements are of widespread distribution, their occurrence having been recognised in Southern Europe, Palestine, India, Egypt and Northern Africa. The type of implement so common in collections falls under the term used by the French to describe them as the *dos rabbatu*. The form has been recognised in implements dating from the early Cave or Aurignacian stage and occurs in all subsequent periods until the introduction of metal.† It is evident, therefore, that from form and style of workmanship alone no date can be assigned for this industry.

* "Les subdivisions du paléolithique supérieur et leur signification." Congrès International d'Anthropologie et d'Archéologie préhistorique, *Compte Rendu de la XIV^{me} Session*. Geneva, 1912. Fig. 6.

† De Morgan, "L'humanité préhistorique," p. 59. 1921

A point of interest lies in the nature of the material used in the manufacture of the flakes. All the pieces but one, which is flint, consist of the black volcanic glass called obsidian. This lava on fracture yields a cutting-edge that is both harder and sharper than flint. Pointed pieces will easily scratch glass, and with difficulty will cut steel. It forms, therefore, a highly efficient material for tools and weapons used alike for cutting, piercing and sawing purposes.

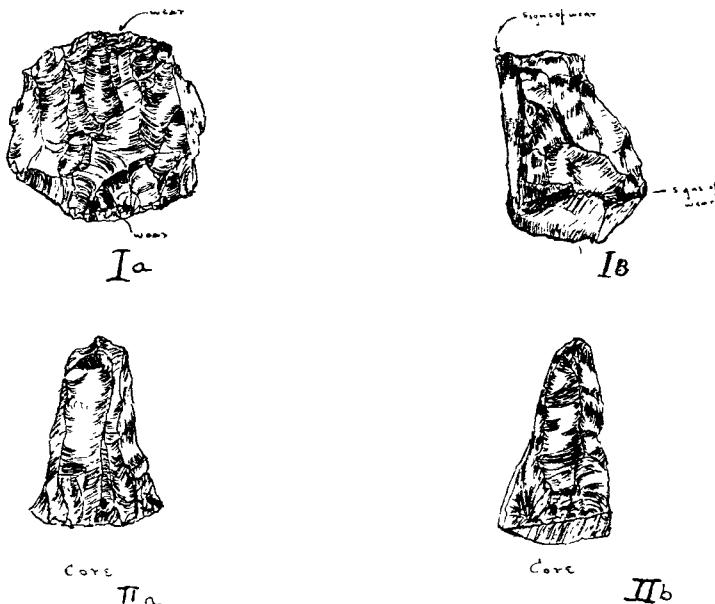


FIG. 3. (F.E.H. del.)

The implements illustrated in Fig. 3 are of great interest and may be described as *grattoirs tartés*. Each has two bases. No. 2 is in the form of a long narrow cone resting on a triangular base; three large flakes and several small ones have been removed in making this specimen.

No. 1 is a double cone, numerous small flakes having pressed off from the base, some apparently as a result of use; the two basal planes are at right angles one to the other (No. 1b).

H. DEWEY.

Ethnology. **Ridgeway.**

Thorn Traps. *By Professor Sir William Ridgeway, Sc.D., F.B.A.*

52

Mr. Henry Balfour's paper (MAN, 1925, 21) on the distribution of traps for fish, wild pigs, etc., made of the terrible *Calamus* thorns, which, as he says, afford one of the most convincing chains of concrete evidence helping to prove an affinity between the culture of the Naga Hills at one end of the range and Melanesian culture at the other end, reminds me of two incidents which occurred in a very different region, but which may serve to illustrate the principle which he has treated so admirably.

1. Fifty years ago when I occasionally frequented race-courses, a country gentleman from my own part of Ireland had his pocket picked at the Curragh Races by a notorious thief. As he had seen the rascal sneaking round him, he had no doubt of the culprit. In the excitement of a race he had lost both his caution and his pocket-book, with a considerable sum in notes in it, and he vowed vengeance. When Punchestown Races were coming on, he matured his plans. Those were the days when men wore "Ulster" coats of frieze or heavy tweed

coming down to their heels, with a belt and very capacious side-pockets. He had the left-hand pocket of his great coat lined thickly with fish hooks, with their points turned down. The day came, and as he was talking in a group of friends, he observed the pickpocket hanging round. He took out his new pocket-book and began to fiddle with it, then casually dropped it into his left pocket and strolled slowly off to watch a race. The thief was after him at once, dipped his hand into the pocket, with the result that his hand was caught both front and back by some dozen hooks firmly stitched in. The knave was afraid to cry out, hoping to free his hand. His captor felt some slight tugs, saw his prey over his shoulder and quickened his pace, making straight to where a head-constable stood with two or three of his men. He pointed to the man, the police seized him at once, but in vain tried to get the wretched fellow's hand out; finally they had to perform the Cæsarian operation on the lining of the coat and cut out the pocket. The captor had not even to waste time and trouble in prosecuting, since the police had caught the rascal *flagrante delicto*.

I wonder if my friend Professor Elliott Smith will admit that this was an independent idea of the King's County squireen, or will he insist that it was a westerly extension from a centre of dispersion in the Naga Hills?

2. In my boyhood at Ballydermott, King's County, my old family home, our shepherd was one James Wolfe, a very clever and good fellow, full of all sorts of country lore. Before I was allowed to carry a gun (at the age of 13), I and my two wire-haired terriers, Fox and Bob, used to go rabbiting with Jemmy and his sheep dogs, Lass and Shep, which were as good after rabbits as my dogs. If a rabbit was cut off from the main burrow and was able to escape into a breeding hole in the open, Jemmy used to cut a long briar from the nearest hedge or brake, clean off the thorns from the thick end to form a safe grip. He then inserted the brier into the burrow, working it along until it would go no further. Thereupon he began to twist the brier until the thorns were well embedded in the rabbit's fur and skin, and then he began to pull out the creature, which had no alternative, except severe pain, but to follow reluctantly until, at the entrance, Jemmy seized it and broke its neck. If this is not actually a case of a "thorn trap," it shows the existence in Ireland of the principle which lies at the bottom of the Asiatic traps—that of catching animals by means of thorns.

WILLIAM RIDGEWAY.

REVIEWS.

India: Folklore.

Tawney, Penzer.

The Ocean of Story. being C. H. Tawney's Translation of Somadeva's Kathā Sarit Sāgara. Now edited with introduction, fresh explanatory notes and terminal essay, by N. M. Penzer, M.A., F.R.G.S., F.G.S. In ten volumes. Vol. II.: with a foreword by Sir George A. Grierson, K.C.I.E., Ph.D., D.Litt. London, privately printed for subscribers only by Chas. J. Sawyer, Ltd., 1924. $10\frac{1}{4} \times 7\frac{1}{4}$. Pp. 375. Price £2 2s.

53

It was my good fortune to be asked to review the first volume of Mr. Penzer's new edition of Mr. Tawney's translation of the *Kathā Sarit Sāgara* for this journal. The second volume now lies before me, and I may say at once that it in nowise falls short of the high standard set by the first volume, and that, like the latter, it is a mine of curious and well-documented information on a variety of matters falling under the general heading of Folklore. The volume is provided with an interesting foreword by the veteran Sir George Grierson, who illuminates some of the passages in the Tales and the notes by personal reminiscences dating back to the years of his service in India.

Thus, in commenting on Mr. Penzer's note on the demon Rahu and the Hindu ideas about eclipses, Sir George Grierson remarks that Rahu is undoubtedly an

aboriginal god, turned by the Indo-Aryans into a demon, who still retains his divine character among the lower classes. In Northern India he is the god of the degraded Dusādhs; in the Central Provinces, the deity of the sweeper caste. Mr. Penzer quotes information supplied by Dr. J. J. Modi, to show that in Bombay Rahu is connected with the Mangs. The point is clearly brought out in Mr. R. E. Enthoven's recently published volume on *Bombay Folklore*, which indicates that Rahu and Ketu are definitely connected in popular belief, not only with the Mangs, but also with the Mahārs and the Bhangis—also degraded tribes, and that it is these three classes only who are the recipients of the secret and open charity which is given at the time of an eclipse. These facts lend additional support to Sir George Grierson's theory. The punishment of nose-cutting, mentioned in the note on p. 88, though usually visited upon an unfaithful wife or mistress, has occasionally been inflicted upon other classes. Some few years ago the residents of a village in the Deccan cut off the noses of three Brahman money-lenders, who had been, in the opinion of the villagers, unduly active in enforcing the repayment of debts. The mutilated money-lenders fled to the nearest town, in which there were a hospital and a civil surgeon, and besought the latter to stitch on the severed fragments of their noses. But mortification had set in, owing to the time which had elapsed since the actual severance, and the three money-lenders had to pass the remainder of their days noseless—a living warning to others that the gentle *rayat* must not be squeezed too hard.

Mr. Penzer has prepared excellent notes on nudity in magic ritual and on precautions taken in the birth-chamber. In regard to the latter, he quotes several examples of the part played by iron in the scaring of evil spirits, and comes to the conclusion that the original cause of the dread of iron by evil spirits is simply that the spirits themselves date back to Stone Age times and still preserve the fears, which all men felt when iron was first discovered and proved its enormous superiority to stone. It is interesting to reflect that in Bombay a burglar's iron tool, if made by the blacksmith and his wife, when both are nude, is believed to ensure the success of the housebreaker. Among the precautions taken in the birth chamber, no reference is made to the custom in vogue among lower-class Muhammadans in India of inviting eunuchs to give rude performances and sing songs at the time of a birth. This is a common feature of life in Bombay, and will possibly be referred to by Mr. Penzer when he deals separately with the question of these human anomalies.

The historical antiquity of gambling, which forms the subject of a note on p. 232, is indicated by the provisions of the *Arthaśāstra* of Kautilya. Though the Mauryan Government regarded gambling as a vice and strictly prohibited it in military camps, it nevertheless appointed a Superintendent of Gambling, whose duty it was to concentrate the practice in central licensed premises and recover for the State 5 per cent. of the winnings of each player, as well as fees for the hire of dice and other apparatus. The note on the sanctity of the Cow in India calls forth an interesting suggestion from Sir George Grierson in connection with the attitude of the Dard tribe of Shins, who live north of Kashmir and are Aryans, though not Indo-Aryans. Both this note and the discussion in Appendix II regarding the origin and distribution of the idea of sovereignty connected with the umbrella will appeal to all who have had experience of India and Burma. In mentioning Šivājī's assumption of the title of Lord of the Royal Umbrella, it might have been added that his descendant, the present Maharaja of Kolhapur, still clings to the title of *Chhatrapatī Mahārāj*.

The volume concludes with an illuminating discussion of the little-known subject of "Poison-damsels." It is observed that Mr. Penzer adheres to the hitherto accepted view that Chandragupta, founder of the Mauryan empire, was of low birth, being an illegitimate son of Nanda, King of Magadha. This view is

based upon the *Mudrā Rākshasa*, a play composed several centuries after the events dramatised. Mr. Harit Krishna Deb has given good reasons for believing that, so far from being a low-caste man, like the later Nandas of evil repute, Chandragupta was legitimately related to the respectable early Nandas. It must, however, be admitted that the point is not free from doubt, and in any case the question of Chandragupta's parentage has merely an indirect bearing on the main subject of Mr. Penzer's final Appendix, which deals with the origin of the legend of the poison-damsel and the manner in which it was carried from the East to Europe. Mr. Penzer's treatment of an obscure subject goes far towards ensuring acceptance of his suggestion that "despite many disadvantages, there is much that is attractive about the poison-damsel."

S. M. E.

Sociology.**Michels.**

Lavoro e Razza. By Roberto Michels. Milan: Dr. Francesco Vallardi. 1924. Pp. 334. Price, 6 lire. **54**

This is an excellent little book on a subject the importance of which can hardly be exaggerated. Its aim is to ascertain so far as possible the significance of race in connection with labour problems, "to remove," as he puts it in the preface, "a very serious difficulty interposed in the way of the right understanding of economic facts arising from the inadequacy of the ideas of most writers shown in their treatment of human labour without taking account of the national differences produced by the most various causes, economic, social, geographical, climatic, ethnic and even purely psychological." From this statement it will be seen that no attempt is made to use the term "race" in any strict anthropological sense, as the author himself expressly points out in a note to the introduction. In his book, he says, the word has no biological and anatomical signification. It is nations and peoples that he has in view, regarded primarily as moral aggregates, groups united by a common will, brought into being partly, it is true, by a rather slender racial influence, but much more through historical, traditional, geographical and other influences. But this application of the idea of race makes his inquiry none the less important and is indeed that which is best fitted to serve the aim kept in view. It is one that necessitates wide and comprehensive surveys, and this makes one of the great merits of the volume.

A very full analytical table of contents enables one to see very readily the plan of the work, but it will be enough to give the subjects of some of the most important chapters. A short introduction is followed by a chapter on the mode in which division of labour between different races is brought about. The fourth chapter deals with the methods of ascertaining the facts of the productivity of national groups. The sixth treats of slave labour considered with reference to its effect (a) on the enslaved peoples, (b) on their masters, and (c) on the labour of liberated slaves. The seventh is the longest and most important chapter in the book. Its subject is the labour of peoples as the result of certain coefficients, viz., (a) physiological conditions; (b) technical equipment; (c) facts connected with race domination, consisting of notes illustrative of the obstacles placed deliberately or otherwise by a dominant race in the way of the economic and moral development of primitive peoples, especially blacks; and (d) psychological factors, among which he includes, for example, "eudæmonistic factors," pointing out the increased returns obtained when joy is combined with work, and quoting a remark of V. Verneuil, a French Governor in Senegambia, who, when watching some Senegalese women at work in clearing the ground, could not help exclaiming, "they amuse themselves as we should amuse ourselves in Europe, they amuse themselves in working" (p. 249).

All these subjects are treated by the author with wide knowledge, with evident conscientiousness and sincerity, and for the most part, it seems to me, with sound

judgment. Occasionally, indeed, one may notice an oversight, as where (p. 28) he accepts without comment from a French writer a statement as to the greatly inferior production of an English as compared with an American miner, leaving out of account the greater ease of working of the coal seams now worked in America, where they are not yet forced back on thin and ill-situated seams. But such oversights are exceptional. It is true that one cannot read the whole book without coming to detect a bias in the author's mind, but it is a bias due to conviction arising from legitimate, though, of course, not infallible, methods of inquiry. The reader must be on his guard, and will equally, of course, have his own more or less justifiable bias. Two general conclusions of the author may be mentioned. First, as regards the general behaviour of the whites to the blacks, he thinks that the charges made against the whites may be summed up thus, that their treatment of coloured races is not merely an offence to their dignity as men, but also leads to the paralysis of forces capable of returning a larger yield or at least arresting their development (p. 222). Second, in the chapter entitled "The Transitory "Character of the Economic Inferiority of Peoples," he asserts (p. 266) that there is no reason to affirm that certain peoples are, in virtue of their race—that is, as the result of their innate physiological and psychological constitutions—subject to an everlasting economic inferiority, although he adds that it is equally impossible to furnish any conclusive proof of the contrary.

In any case the book contains a great multitude of important facts, and the author's contentions, whether right or wrong, seem to be worthy of earnest study, and it is much to be wished that the book should appear in an English translation. If it does, there are two slips of the author that are not likely to be repeated in that translation. On p. 143 he speaks of Mr. John H. Harris as a Negro writer, and on p. 219 he mentions the Hon. Bertrand Russell as "Lord" Russell.

GEO. G. CHISHOLM.

Europe: Archæology.

Burkitt.

Prehistory; a Study of Early Cultures in Europe and the Mediterranean Basin. By M. C. Burkitt, M.A., F.S.A., F.G.S., with a short preface by l'Abbé Breuil, Professor at the Institute of Human Palæontology, Paris. Second Edition. Cambridge: at the University Press. Pp. xxvi + 438. Price 35s. net. **55**

After four years this useful work, which was reviewed in MAN, 1921, 112, has run into a second edition. The author has taken this opportunity of adding notes at the ends of the first four chapters and to Chapters 12 and 22; he has also recast chapter 13 and added a map showing the more important localities cited in Western Europe.

The most important additions, however, appear in the preface to the new edition. Here he notes the depression of the land in Chellean times and its uplift in Mousterian times. He also gives, at the end of Chapter 2, an all-too-brief summary of Professor Sollas's account of the work of Depéret and Lamothe, showing that the raised beaches around the Mediterranean contain a warm fauna. He does not, it would appear, conclude from these facts that the land was depressed in interglacial and uplifted in glacial times, though on p. 20 he quotes the view of the late Professor McKenny Hughes that such movements might have been the cause of the Ice Age.

He advances further evidence for the multiglacial view, though, strangely enough, he omits any reference to the most important paper on the subject, that published in our *Journal* by Professor Boswell and Mr. Reid Moir. He is still in doubt as to the age of East Spanish Art, and make no reference to the arguments adduced by Professor Obermaier to show that they are late Palæolithic in date. But, in spite of these omissions, the volume retains its usefulness as a good summary of the Palæolithic Age.

H. J. E. P.

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RAM NARAYAN
Chet & Foreign
P.O. 1165, New York, N.Y.

PLATE G.

MAN, JULY, 1925.



ALFRED CORT HADDON, M.A., Sc.D., F.R.S.

From a portrait by P. A. de László.

ORIGINAL ARTICLES.
With Plate G.

Anniversary.

Fegan.

Dr. Haddon's Birthday Celebrations. *By Miss Ethel S. Fegan.*

56

On Saturday, May 23rd, 1925, a dinner was held in Christ's College Hall, Cambridge, in honour of Dr. Haddon, on his attaining his seventieth birthday. As Members of the Institute know, a fund has been raised for his portrait, and this was presented to him at the dinner, together with a list of the subscribers, by the Chairman of the Fund, Professor Sir William Ridgeway. The portrait has been painted by Mr. P. A. de László, and will be hung in Christ's College; and with great generosity the artist has painted and presented another portrait, which will find its home in the Museum of Archaeology and of Ethnology in Cambridge, with which Dr. Haddon has been so long connected, and to which he has given the bulk of his Torres Straits and New Guinea collections.

It is pleasant to record that a very representative body of friends has subscribed to the portrait, from all parts of the world, both from our own Dominions and Colonies and from foreign countries, and many friends in the British Isles made an effort to be present at the dinner. The guests included the Vice-Chancellor of the University, the Master and Fellows of Christ's College, Professors Marr, Stanley Gardiner, Nuttall, T. J. Wilson, Pearson, the Registrar, Dr. Duckworth, Dr. Minns, Dr. Cook, Mr. Brindley, Mr. Henry Balfour, Dr. C. S. Myers, Professor C. G. Seligman, Professor J. Graham Kerr (Glasgow), Professor A. F. Dixon (Dublin), Mrs. Haddon and her two daughters, the Mistress of Girton, Dr. Mabel Slater of Newnham, Miss Allan, Lady Gomme, Mrs. Fay (Toronto), many of the wives of the Fellows of Christ's, the artist and Mrs. de László, Mr. Martin White, Mr. Walter Haddon, Mr. H. J. Rose and many others, 100 in all. The College plate was set out on the tables, which were also decorated with pink carnations. The *menu* card was adorned with a typical Papuan Gulf design, and speeches in honour of Dr. Haddon were made by Sir William Ridgeway, who presented the portrait, Mr. Henry Balfour and Professor C. G. Seligman. Sir William Ridgeway spoke of Dr. Haddon as the inspirer of other men, infecting them with his own enthusiasm for his subject; Mr. Balfour called him a kind of anthropological trinity, equally at home in the three great divisions of Physical Anthropology, Social Anthropology, and Technology; and Professor Seligman spoke in warm terms from his personal experience of him as a teacher and comrade in the field. After Dr. Haddon's reply, the Vice-Chancellor proposed the health of the Master and Fellows, and Dr. W. L. H. Duckworth proposed a vote of thanks to the Chairman, to whose unremitting labours much of the success of the whole undertaking was due.

The portrait for the College represents Dr. Haddon in his scarlet gown, and it is hoped that a reproduction of this portrait will be sent to all subscribers. In the portrait for the Museum (Plate G), Dr. Haddon is in his ordinary doctor's gown and is caressing a skull. All who have attended his lectures on Physical Anthropology will recognise this as specially happy in thought, and the many who have enjoyed his books will feel the suitability of the attribute for such an inveterate "head-hunter."

On Monday afternoon, Dr. and Mrs. Haddon held a garden party in the Fellows' Garden of Christ's College, which gave still more friends an opportunity of seeing the portraits.

ETHEL S. FEGAN.

Obituary.

Haddon.

Henry Ling Roth, born February 3rd, 1855; died May 12th, 1925.
By A. C. Haddon, Sc.D., F.R.S.

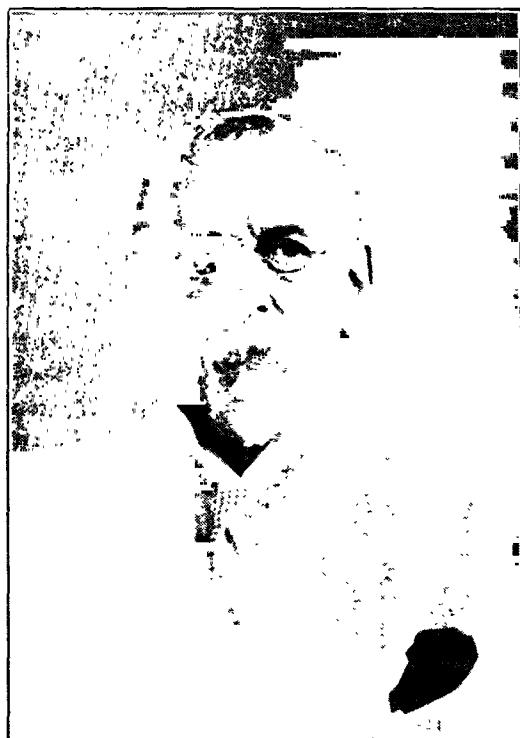
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Our science and the Royal Anthropological Institute have suffered a great loss by the death of Mr. H. Ling Roth on May 12; his wife and two sons survive him. He was born on February 3, 1855, being a son of Dr. Mathias Roth, of Harley

Street, London, who introduced physical culture into England for educational purposes and for the alleviation of bodily deformities. Four of his five brothers were distinguished medical men, one of whom, the late Dr. F. Norman Roth, Medical Officer to the Benin Punitive Expedition, gave our late Fellow valuable information on Benin, and Dr. Walter Roth is well known to us for his valuable researches in the ethnography of North Queensland and of British Guiana, where he is now residing. Mr. Ling Roth was educated at University College School, and studied Natural Science and Philosophy in Germany; he travelled in Australia, the West Indies, and over most of Europe, including Russia, and finally settled in business in Halifax in 1888.

Mr. Ling Roth devoted the whole of his spare time to ethnological studies, which were of very wide range. In June, 1900, he was appointed Honorary Curator of the Bankfield Museum, which was then in a chaotic condition; it was unattractive and but seldom visited.

In 1912 he was appointed half-time Keeper and later gave his whole time to the Museum: his health, which was never robust, broke down and in August, 1924, he sent in his resignation and was succeeded by Mr. G. R. Carline, but, characteristically, he continued to work in the Museum till March 31 of this year. I have had the opportunity of visiting Halifax Museum on various occasions, and those who have done the same will acknowledge that it has gradually been transformed through the untiring efforts of Ling Roth into an ideal museum. His interest in antiquarian lore enabled him to illustrate the growth of Halifax and the development of its special industries. He accumulated a remarkable series of appliances to demonstrate the progress of the textile industries, and further illustrated this by specimens from various parts of the world, a



HENRY LING ROTH.

(By courtesy of the Editor of *The Halifax Courier*.)

legitimate expansion for an essentially local museum. General ethnology has not been neglected, but this is treated rationally, the more important of such exhibitions illustrate primitive trading and currency, steelyards and bismars, quill-work, beads and bead-work, stimulants and narcotics, time-reckoning, transport, fire-making and illuminants, musical instruments, the evolution of weapons, pottery, and the like. The specimens are carefully chosen, well arranged and most admirably labelled, and the interpolation of photographs and engravings adds considerably to their interest, so that the Museum is a teaching institution of prime importance, and is visited by great numbers of school children. Not only has Mr. Ling Roth given time, knowledge and skill to the Museum, but he has enriched it with many donations.

For a great many years I have enjoyed the friendship of Ling Roth, who had an affectionate and sympathetic nature and was always ready to help those who

needed information; his opinion on social matters was highly valued by those who sought it, as he had an unbiased religious and political outlook. He was a master of the art of collating information and of presenting scattered records in a readable form, which have been of great assistance to his fellow students: his well-known monographs on the aborigines of Tasmania and the natives of Sarawak and British North Borneo will always remain as standard works. He invariably took the greatest pains to acquire information upon subjects upon which he was working, and his publications are characterised by accuracy and concise clarity as well as by apposite illustrations. He was a very clever draughtsman and his drawings bring out just those details which are essential, his books and papers are remarkable for their wealth of illustrations, which must have cost him an enormous amount of time and trouble, as well as considerable expense, for, though a poor man, he never spared expense where his work was concerned; and the Civil Pension which he received in recognition of his monograph "The Aborigines of Tasmania" was used for the expenses connected with producing his books and papers: for example, the "Bankfield Museum Notes" were issued at his sole cost. Ling Roth was one of those quiet, unpretentious, conscientious workers who get through an immense amount of work, but who too frequently, as in his case, do not receive full recognition of their labour until it is too late.

I am greatly indebted to Mr. Carline for substantial help in collecting the following list of Ling Roth's publications, and to Miss E. S. Fegan, of Girton College, for preparing the list for press.

A. C. HADDON.

PUBLICATIONS OF H. LING ROTH.

- 1878. Notes on the Agriculture and Peasantry of Eastern Russia. London.
- 1880. A report on the Sugar Industry in Queensland.
- 1881. The Climate of Mackay. (*Proc. R. Soc., N. S. Wales.*)
- 1882. Notes on Continental Irrigation. London.
- 1883. On the Roots of the Sugar Cane. (*Journ. R. Soc., N.S.W.*)
The Sugar Industry in Queensland. Queensland.
- 1884. The Mackay Queensland Sugar Crop, as affected by weather and want of labour. (*Brisbane Courier*, Apr. 1.)
Notes on the Habits of some Australian *Hymenoptera Aculeata*. (*Journ. Linn. Soc. Zool.*, xviii.)
- 1885. Arbère: a short contribution to the study of Peasant Proprietorship. (*Journ. Stat. Soc.*)
Franco-Swiss Dairy Farming. (*Journ. R. Agric. Soc.*)
The Animal Parasites of the Sugar-cane. (*Sugar Cane*, March and April.)
Further remarks on the Roots of the Sugar Cane. (*Timehri*, June.)
- 1886. Addenda and index to the Animal Parasites of the Sugar-cane. (*Sugar Cane*, February.)
Prof. A. de Candolle on the Origin of the Sugar Cane (trans. of "Origine des Plantes cultivées," with notes.) (*Sugar Cane*, April.)
- 1887. The Origin of Agriculture. (*J.A.I.*, XVI.)
Australian Tunes. (*J.A.I.*, XVI.)
The Aborigines of Hispaniola. (*J.A.I.*, XVI.)
- 1888. Bibliography and Cartography of Hispaniola (*Suppl. Papers, R. Geog. Soc.* II.)
- 1890. On Salutations (*J.A.I.*, XIX.)
A guide to the Literature of Sugar. London.
The Aborigines of Tasmania. London; 2nd ed. Halifax, 1899.
The Philosophy of Red Hair. (Pamph.)
- 1891. Crozet's Voyage to Tasmania, New Zealand, etc. (trans.). London.

1892-3. The Natives of Borneo, ed. from papers of Brook Low, 2 pts. (*J.A.I.*, XXI, XXII.)

1893. On the Significance of Couvade. (*J.A.I.*, XXII.)

1896. Negritois in Borneo. (*J.A.I.*, XXV.)

The Natives of Sarawak and British North Borneo. London. 2 vols.

Alleged native writing in Borneo. *Intern. Archiv. f. Ethnogr.*

1898. Is Mrs. F. C. Smith a "last living Aboriginal of Tasmania"? (*J.A.I.*, XXVII.)

Spears and other articles from the Solomon Islands. (*I.A.f.E.*)

The Whithorn. (*I.A.f.E.*)

Notes on Benin Customs. (*I.A.f.E.*)

1898-9. Notes on the Jekris, Sobos and Ijos of the Warri District of the Niger Coast Protectorate, by R. K. Granville and F. N. Roth, prepared by H. Ling Roth. (*J.A.I.*, XXVIII.)

Stray articles from Benin. (*I.A.f.E.*)

1900. Artificial Skin Marking in the Sandwich Islands. (*I.A.f.E.*)

Note on a *Hkoung beht* set. Burma. (*J.A.I.*, XXX, *Anth. Rev. and Misc.*, 64.)

On permanent Artificial Skin Marks: a definition of terms (paper at Brit. Assn.) (*J.A.I.*, XXX, *Anth. Rev. and Misc.*, 117.)

1901. Maori Tatoo and Moko. (*J.A.I.*, XXXI.)

1900-1. The Fijian Collection. (*Halifax Naturalist.*)

1901. The Burmese Collection. (*Halifax Naturalist.*)

1903. Great Benin: its customs, art, and horrors. Halifax.

1905. Tatoo in the Society Islands. (*J.R.A.I.*, XXXV.)

Tatoo in Tunis. (*Man.*, V. 72.)

Tatoo in Tierra del Fuego. (*Man.*, V. 90.)

1906. Tonga Islanders' Skin Marking. (*Man.*, VI. 4.)

The Yorkshire Coiners, 1767-1783; with notes on Old and Prehistoric Halifax. Halifax.

1908. The Discovery and Settlement of Port Mackay, Queensland. Halifax.

Mocassins and their Quill-work. (*J.R.A.I.*, XXXVIII.)

1910. Oriental Silverwork, Malay and Chinese. London.

1911. On the use and display of Anthropological Collections in Museums. (*Museums Journ.*, X.)

1912. Oriental Steelyards and Bismars. (*J.R.A.I.*, XLII.)

1914. The Genesis of Banking in Halifax. Halifax.

1915. Bankfield Museum publications. (*Museums Journ.*, XIV.)

1916. Sketches and Reminiscences from Queensland, Russia and elsewhere (privately printed).

Observations on the growth and habits of the Stick Insect, *Carausius morosus*, Br. (*Trans. Ent. Soc.*, London.)

A Loom from Iquitos [Upper Amazon]. (*Man.*, XVI, 62.)

1918. Notes on some large Stone Implements from Queensland. (*Man.*, XVIII, 10.)

1919. American Quill-work. (*Man.*, XIX, 5.)

1920. Some unrecorded Maori Decorative Work. (*Man.*, XX, 39.)

1921. Models of Egyptian looms [from Thebes]. (*Anc. Egypt*, pt. IV.)

1923. The Maori Mantle. Halifax, (limited to 120 copies.)

Some Tatoo notes from the Pacific.

American Quill-work: a Clue to its Origin. (*Man.*, XXIII, 72.)

1922. Were the Ancient Egyptians conversant with Tablet-weaving (*Brettchen-weberi, Tissage aux Cartons*)? [With Grace M. Crowfoot]. (*Liverpool Ann. Arch. & Anthropol.*, X, Nos. 1 & 2.)

n.d. Some Experiences of an Engineer Doctor, by F. Norman Roth; ed. by H. Ling Roth (privately printed).

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France: Archæology.

Collum.

Menhirs and Burials. By Miss V. C. C. Collum.

58

In further confirmation of Mr. C. Daryll Forde's note (MAN, 1924, 133), stating that Professor Peet (discussing the Megalithic Civilisation in France, in Vol. II of the "Cambridge Ancient History"), is not correct when he says "menhirs . . . have no connection with burials," comes the interesting discovery, made in 1922 by Zacharie le Rouzic and Monsieur and Madame Saint-Just Péquart (see *Carnac : Fouilles faites dans la Région. Campagne, 1922* : published in 1923 by Berger-Levrault, Paris, at 25 francs), that the alignments of Kermario (= "city of the dead"), near Carnac, in the Morbihan, had been carried right over a previously existing necropolis at the locality known as Er Manieù, indicated by a menhir standing on the summit of the mound and incorporated into the aligned stones. This menhir (above ground) is 3·50 metres high and faces north-south, whereas the stones of the alignments of Kermario have dwindled in height, at Er Manieù, to from two to three feet; neither is it aligned with any of the rows of stones, which run east-west. James Miln pointed out in 1881 (the year of his death) that occasional menhirs, differently orientated, and marking the site of irregularly circular primitive constructions, were included in the Alignments of Carnac, just as the alignments of Kermario themselves had later been incorporated into the field, first of Celtic, then of Roman, defence works. (See *Les Alignments de Kermario : Fouilles faites à Carnac*. Rennes, 1881.) Dr. Capitan, Vice-President of the State Commission on Prehistoric Monuments, agreeing with le Rouzic that this menhir was obviously indicative of a constructed mound over which the alignments had been carried, gave Government permission for excavations to be undertaken. These revealed that the mound covered an oblong shaped area measuring 35 metres in length by 16 at its eastern and only 11 at its western extremity, and surrounded by a roughly made wall, itself buried beneath the mound. In the mound were discovered some 50 constructions of varying types, of which 20 were excavated, and the remainder

left untouched for the information of future generations. Measurements and photographs were taken at every stage of the work. Some of these incontestably protected small sacred hearths, in which fragments of woodash, chiefly oak (*Quercus sessiliflora* Sm.) were found. Beside the menhir was a betyl, 80 cms. high, planted in clay.* Almost immediately beneath the menhir (which the excavators carefully propped up in its existing position when they had dug down, at one side, to its base at 88 cms. below the surface) was a tomb-like construction, built over a trench 1 metre 45 cms. long by 45 cms. broad and 50 cms. deep, filled with light black earth which had mixed with it fragments of wood charcoal, and from which was recovered broken pieces of a rough earthen vase and some flints. The construction itself consisted of a large table of stone measuring 3 metres by 1.90, and 80 cms. at its greatest thickness, lying east-west on a roughly made wall, 60 cms.



FIG. 1.—THE MENHIR SEEN FROM THE WEST, AFTER THE STONE HAD BEEN RESTORED TO ITS ORIGINAL STATE. THE TOMB LIES UNDER THE GROUND, SLIGHTLY TO THE RIGHT OF THE MENHIR. (ENTRANCE AT THE EAST.)

thick and 55 cms. high, circumscribing a cavity 1.15 metres wide and 3.55 metres long, filled up, except for the trench below, with a compact mass of clayey earth. On the southern portion of the wall on which the table rested, and beneath the table, were discovered the fragments of a vase in fine grey earth. The table was carefully covered with stones.

Roughly graven on the southern portion of the table of stone was an axe-head 35 cms. long, with handle 60 cms. long, and, on the base of the menhir, undulating figures resembling serpents standing on their tails (figures which are represented on several of the stones at Gavr'Inis), together with, on one face of

* It may not be irrelevant here to call attention to the standing stone found inside Carn F. at Carrowkeel, Co. Sligo, described in Vol. XXIX, Section C, No. 9 of the *Proceedings of the Roy. Irish Acad.*, Jan. 1912, by Macalister, Armstrong and Praeger.

the menhir, a symbol resembling the sun with rays, graven on the ovate shield of the head stone in the famous dolmen known as the "Table des Marchands." Some carefully placed small axe-heads in diorite and fibrolite were also found at the foot of the menhir. (I saw the "serpents" and the rayed sun quite distinctly in August, 1923, by the light of matches—a subterranean chamber has been constructed so that the base of the menhir and the tomb may be examined—but neither Monsieur le Rouzic nor myself could see either of them, by the light of an electric torch, when we visited the menhir in October this year. Excellent casts and photographs, however, taken when the menhir was excavated, exist.)

In construction No. 52 just outside and almost in contact with the north-western buried boundary wall of the oblong space covered by the mound, where, in plan, it deviates from the straight and widens out, and at about 4 metres distance from the north-west extremity of the table stone of the principal monument, and built of four flat stones planted on end and containing a box-like cavity measuring 34 cms. by 26, covered over by three concentric layers of flat stones, laid on end and inclined towards the summit, there were found, in the black earth with which the cavity was filled, two complete feetless vases, which, though broken by the weight of earth and stones covering them, had obviously been placed there intact, the one inside the other. No. 1, a brown earthen vase, measured 175 mm. in height, 157 mm. in diameter at its neck, and 21 cms. in diameter at its belly, which was distinctly carinate. To quote from le Rouzic's report: "In form it constitutes a very fine specimen of the type known as the "*vase caréné*." No. 2. of the same paste, was 158 mm. in height, 84 mm. in diameter at its opening, and 12 cm. at its belly, being pear-shaped, a rare form, le Rouzic remarks, in the dolmens of the Carnac district. "The presence in this "mound of articles similar to those which are to be found in the dolmens of the "district, though less numerous, permit us to conclude," says le Rouzic, "that "the date of the construction of this monument is not far removed from the date "at which these megaliths were erected. Some among them may even be "contemporary, and any peculiarities may possibly be due merely to the fact "that they were erected for different purposes. The discovery of a carinate "vase—a form that archaeologists agree in placing in the Bronze Age—confirms "the opinion we have always held that the dolmens of this district belong to that "age. In any case, the certain conclusion we may draw from the results of these "excavations is the uncontested anteriority of the mound to the Alignments "which are carried over it" (p. 115 of the Report referred to). The very fact that the alignments were carried over such an important mound and that the menhir was thus incorporated into the lines of stones, would suggest that the personage whose funerary rites were here commemorated had already been forgotten (or belonged to a different people) when the alignments were constructed.

Although no actual burial is indicated by this menhir, it does obviously indicate a mound built over a sacred place of incineration of some sort, and the probability surely is that these were incineration rites connected with the funeral of some great personage. The subsidiary constructions are not all of the same day, but are obviously of the same period from the style of their architecture. The mound was built over the whole lot of them.

I have seen several other menhirs indicating mounds of this elongated kind incorporated in the Carnac alignments, and, if the French Government permits further investigation, more light may be thrown on the relation in which the older single menhirs stand to the later alignments, and in which both stand to the dolmens of the district.

Le Rouzie, and his fellow excavators, Mons. and Madame Saint-Just Péquart, have now in the press a volume in which, with the aid of the French Government, they are publishing reproductions of all the signs—at present known—graven on menhirs and on the stones of dolmens and cromlechs throughout the Morbihan.

V. C. C. COLLUM.

America, South: Archæology.

Hornell.

The Archaic Sculptured Rocks and Stone Implements of Gorgona Island, South America. By James Hornell, F.R.A.I., F.L.S., Ethnologist to the "St. George" Expedition. (Continued from MAN, 1925, 48.)

The second series of sculptured rocks is less easy to describe. They lie low down on the shore, and are exposed. The graven figures, which are greatly

worn and seldom decipherable, are extremely numerous; practically every boulder within a radius of 20 feet from one great central block exhibits traces of incised carving. Frequently several are visible on the same stone. The pivot stone, a massive, round-backed boulder, 7 feet long by $4\frac{1}{2}$ feet wide and $2\frac{1}{2}$ feet high above ground, is particularly important. The seaward or east side in the upper part at either end is thickly sprinkled with closely set small circular depressions or "cups," from one to one and a half inch in diameter and half an inch in depth. Near the ground level at the north end of the same face is the figure of a small bird, about 18 inches

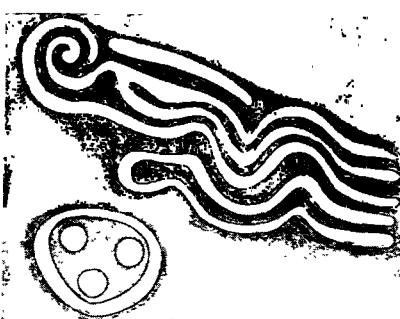


FIG. 3.—CURVILINEAR DESIGN ON BOULDER. GORGONA.

long, with beak wide open; possibly a booby (*Sula*, sp.), a species abundant in the island. At the other end of the stone, low down, is another small figure, 9 inches in length, apparently representing a frog. Finally, on the landward or west side is a vigorously executed graving of a triangular-bladed axe, lashed to a haft.

The cutting edge of the axe measures 7 inches.

Adjoining this boulder on the south aspect was a small ridged stone (since removed) having a rude human (?) face, of the usual sub triangular form and three pits for the features, carved at each end of the axial ridge. On the sloping seaward face, oriented eastward and filling up the space between the two faces, a distance of two feet, is a peculiarly symmetric device (Fig. 3), consisting of a number of superimposed groovings in the form of horizontal brackets, the uppermost two passing laterally into a coiled scroll terminal turned upwards.

On a large stone alongside, a figure of a monkey, squatting on its haunches, is seen. Another carving on the same rock has the same general outline and probably was a companion figure; the details are too worn to permit of certainty.

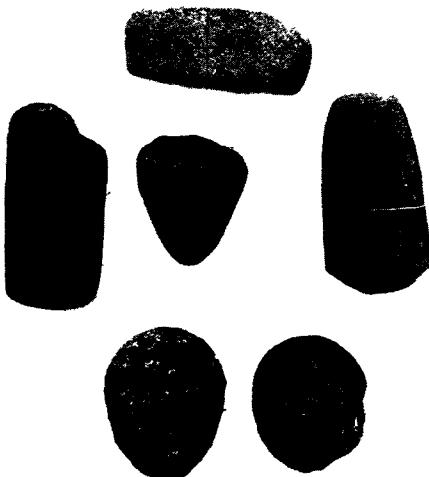


FIG. 4.—STONE WEAPONS, ETC. GORGONA.

On many of the other rocks at this place are cup or pit markings and the remains of numerous carved designs, some evidently human (?) faces, others coiled groovings such as are seen in the arms of the "Sun-god" figures at the first beach, and some unmistakable rays or ray groovings proceeding from carvings otherwise unrecognisable through weathering.

From the coarse character of the broad rounded channels which form the outlines of all these figures, and the great age bespoken by the weather-worn condition of all these that have not had some measure of protection by being embedded in sand or gravel, I conclude that stone implements were employed in their execution; had metal tools been used the graven outlines would have been finer, and more sharply defined. As it happens, stone implements were actually found in fair numbers on and among the shore shingle in the vicinity of both these series of sculptured rocks (Figs. 4, 5); a stone axe was also discovered *in situ* in clayey soil, 16 inches below the present land surface and within two inches of an inverted pot, complete, but broken into fragments. Those found loose in littoral gravel have evidently been but recently washed out of the old land surface now being eaten into by the sea on this side of the island. The principal of these stone implements are four axe heads (celts) of fine grained rock, all approximately 6 inches

in original length, one sub-triangular axe-head, 3½ inches long, two hand hammers of chalcedony chipped to a rounded form, one thin chisel-shaped scraper, two small discs perforated in the centre, four thick discs, some round, some elliptical, probably used for sharpening or rubbing down other objects, one fine grained thin slate disc, about four inches in diameter, possibly used as a palette; in addition there were numerous fragments of stone troughs found embedded in the beach near where the majority of the celts were found. These troughs are roughly shaped

FIG. 5.—STONE IMPLEMENTS. GORGONA.

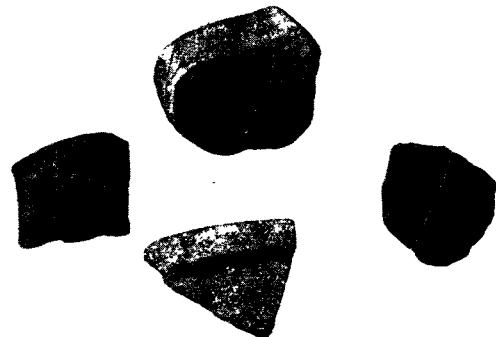
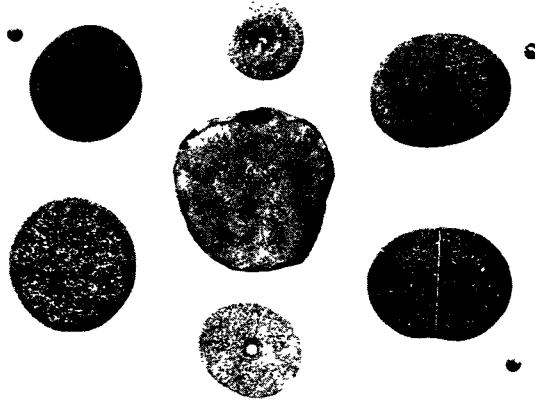


FIG. 6.—POTSHERDS. GORGONA.

out of dolerite blocks. The size varies; that of one large fragment that we brought away, is one foot high and one foot five inches wide at the higher end; the cavity within is 10 inches wide by 6 inches deep, the angles rounded. The other end, missing, was evidently lower, for both the floor of the cavity and its side walls slope downwards from the end of the fragment described. Like all the others that we saw, this trough appears to have been broken through at about mid-length.

It is reasonable to connect the users of these stone implements with the gravers of the sculptured figures on the shore boulders; if so, then the latter must be of Neolithic age.

Fragments of pottery are common in the old soil, 12 to 18 inches below the surface. A complete pot, inverted, but broken into many small pieces, which was found had a rounded bottom and thick everted lip. It was quite plain, without distinctive markings. Three other fragments show circular ridges. In one case it is clear that the low raised ring (2.5 mm. high) was used as a basal ring stand; the fragment is sufficient to reconstruct the original shape and to enable us to see that it belonged to a shallow bowl-shaped vessel (Fig. 6). In another case the projecting rim is nearly an inch in height, with an outside diameter of 2½ inches. It probably belonged to a vase of the form of two from Nicoya, now in the National Museum of Costa Rica.* In the third fragment, the ridge is low and worn and is most likely a basal ring as in first of these three. Among the odd sherds found, one only displayed any pattern (Fig. 6). Here a beading of small quadrangular cushion-shaped projections have been formed on the neck or shoulder of the pot. Another

fragment showed traces of red pigment, burnt in. The composition and appearance of these fragments vary considerably. In the case of the inverted pot, the sherds are burnt red throughout; in others they are red or yellow on both surfaces and black within. In a few, notably the one with a beaded pattern, the pottery is black in colour, with tiny points of white scattered throughout, due to impurities in the clay used. These last are much harder than those red in colour.

The four graven stones of relatively modern age occur close to the second series of sculptured boulders at the south end of Watering Bay, but all are high up on the beach, just below the limit of ordinary high tide. The most modern of the series is one inscribed with the name and date, deeply incised:—F. D. Croz, 1847—the name, I believe, of a former planter. Rather older is

FIG. 7.—HEAD INCISED ON ROCK. GORGONA.

an incomplete but finely incised representation of a three-masted ship, probably executed in the first half of the last century; considerably older, probably by a century, is a full-length figure of an officer in uniform, 4 feet in height. Pizarro and twelve companions spent seven months on the island in 1527 (Preseott's *History of the Conquest of Peru*, Book II, Chap. IV), but neither the dress nor the features permit of identification with the Conquistador. It may represent the privateer Woodes Rogers, who spent some time at Gorgona in 1709 careening his ships.

Last of the sculptured rocks is one on a flat rock surface, level with the surrounding shingle, which represents the full face of a man on whose head is set a close-fitting cap ending in a "tail" of most curious form and of extraordinary length, proceeding from the summit (Fig. 7). The face is somewhat long, while the features are regular and beautifully moulded: the nose is long and distinctly wide at the base, due to the inflated form of the nostrils. No suggestion of obliquity is seen in the eyes, which, with the mouth and ears, are finely proportioned. The head is set on a

* Guardia, R. F., *History of the Discovery and Conquest of Costa Rica* (translation). New York, 1913.

neck strong and rather thick. The cap is most peculiar. It would seem to be of cloth or possibly of knitted material. The "tail," emerging from the summit, takes a wide curve outwards and downwards till it reaches the level of the right ear; it then bends inwards and turns upwards and outwards, forming a loop; from this point it ascends till on a level with the top of the cap, and finally turns outwards again to end some distance from the head and directed away from it, with a snake's head as a terminal ornament. This curious substitute for a tassel is carefully graved, the mouth open and the tongue thrust out. The grotesque headgear suggests an Indian origin—the features might well be those of an Inca chief. Near the lower edge of the stone as we look at the face and somewhat below and to the left of the neck is a symmetric and finely formed outline of a five-pointed star with a radius of four inches. If I am right in placing the age of the hatted European figure as of the first half of the eighteenth century, this sculptured head covered with its bizarre cap must be considerably older; the incised lines are distinctly less sharp and deep and show clearly the defacing effects produced by the attrition of the sand and pebbles that surround and partially overlie the stone on which they are graved.

The three last mentioned figures have undoubtedly been engraved with a metal chisel. The incised lines are all sharp and clear, narrow and shallow, wholly unlike the coarse broad gutter-like outlines of the archaic figures. That they belong to periods widely separated in point of time is one of the few positive facts we can at present be sure of. So far as I am at present aware—I write far from any civilised centre and without opportunities for reference—no other series in any way comparable with these extraordinary and mysterious carvings exists elsewhere in South America. To seek any parallel one has to turn to the Old World.

JAMES HORNELL.

P.S.—As a result of further investigation on a subsequent visit to the island it would appear that the group of graven boulders of the first Group appear to have been the central and all-important sacred place of the island. The second series of sculptured stones seems to belong to a different and probably secular category. The subjects chosen for treatment are comparatively trivial and form a very miscellaneous collection. Besides those enumerated in the first account of these remains, were, I have found, an extremely well-preserved graving of a crested bird, probably a crested curassow, a human head bearing two curved plumes, sinuous figures possibly representing sacred snakes, two multi-rayed stars and a human (?) figure with arms widely extended.

Much of my time on the island was devoted to digging in two settlement sites on the east of the island. I was fortunate to find *in situ* stone axes, spindle whorls, and mealing stones, etc., associated with potsherds of characteristic form. Some of these were dug from a horizon 2 feet 9 inches below the surface. I was also able to find a good many pieces of decorated pottery in simple designs. No metal was discovered, but I heard from a Spanish gentleman that fragments of gold had been picked up on the adjacent sandspit occasionally. When I say no metal, I must except a piece of iron oxide—possibly haematite—which seems to have been the source of the red pigment used to paint the pottery. There is good clay for pot-making at both sites and I believe these people were expert potters. These remains must be very old, for the stratum in which some are is nearly three feet below the present land surface and the overlying material is a stiff yellow clay, different from the loamy nature of the layer containing the remains. There is no local tradition of any former occupation.

From the position of the two sites, it is practically certain that these people lived in pile-dwellings. On the adjacent mainland, under similar conditions, the present-day Indians have this habit.

Spain : Archæology.

Howes.

A Galician Rocking Stone. *By H. W. Howes.***60**

The north-west corner of Spain is a region abounding in stone monuments. As might be expected, there are many remains of ancient stone-worship, not least the cult of St. James. The pilgrims who, in the Middle Ages, flocked to his shrine at Santiago de Compostela made a point of visiting Mugia, near Cape Finisterre, in order to see the famous rocking-stone of the Barca, or, to give it its full designation, "Our Lady of the Barca." The word "Barca" means "boat," and refers to the pious tradition that the Virgin came in it on a certain occasion in order to visit St. James. Composed of granite, the principal formation of the rocks in the vicinity, it is tabular in form. In length it measures about 9 metres; in breadth, 7 metres; in height, a little under half a metre. The question arises as to whether it was placed in position by human agency, or whether it is the result of weathering. I am of opinion that the stone is a section of rock that was naturally loosened and then adjusted by men in order to cause it to oscillate. Confirmation of this view is provided by the fact that there are distinct evidences of working on the lower face. The point of balance cannot be seen, though one person, if he presses at the correct spot, can move it easily. The pilgrims, who came thither on the day of the Blessed Virgin, not only endeavoured to cause the stone to rock, but danced on it.

H. W. HOWES.

REVIEWS.

Greenland, Denmark : Archæology, Physical Anthropology.

Hansen, Jónsson, Nörlund.

1. *Meddelelser om Grønland*, Bind lxvii. Nr. 1. Dr. Poul Nörlund; *Buried Norsemen at Herjolfsnes*. (270 pp.) Nr. 2. Professor F. Jónsson; *Interpretation of the Runic Inscriptions*. (18 pp.) Nr. 3. Professor F. C. C. Hansen; *Anthropologia Medico-historica Groenlandiae antiquae* (in English). (255 pp., 85 plates.) 1924.

61

2. *Outlines of the Geography and History of Greenland for the use of Anthropologists*. By F. C. C. Hansen. Folio, 40 pp., 6 maps. (Crania Groenlandica, 1915.)

3. *The Sagittal Section*. By F. C. C. Hansen. Folio, 34 pp. (Crania Groenlandica, 1915.)

4. *De ældste Kongegrave og Bispegrave i Roskilde Domkirke*. By F. C. C. Hansen. Folio, 85 pp., 6 plates. 1914.

5. *Identifikation og Rekonstruktion af historiske Personers . . . af skelettet*. (Københavns Universitet Festschrift, 1921). By F. C. C. Hansen.

Fine work such as that issued by Professor Hansen and his friends is a joy to see. (1) The expedition to recover the history of the Norse settlements in Greenland is recorded (in English) in the fullest manner, with abundant illustration of the graves, the complete dresses which were carefully extracted, the copies of contemporary mediæval sculpture and MSS. for comparison, the skeletal detail indicating modes of life, and the restoration of living form from the skulls. The dresses are probably the most varied and complete that exist of the twelfth and fourteenth centuries. The frozen soil has kept them better than in any other situation. They are of unexpected historic value, as the liripipe hood, which might be assumed to be only a fashionable vagary, is found repeatedly. If thus worn in a most remote and dreary settlement, it might well be worn by the Norse raiders, and even be the source of the hoods in Professor Dart's Bushman drawings.* While noting this work it is well to call attention to previous issues of the expedition results. (2) The geography and history of Greenland, with maps of the settled

* *Nature*, March 21, 1925, p. 425,

districts, is a full account, without passing into disproportionate detail, but stating all the stages of discovery. (3) The Sagittal section deals with a mode of comparison of skulls, by taking the angles between lines joining fiducial points. This raises the question whether we should measure what is easiest to define, or seek for single elements of growth.

The account (4) of the royal and episcopal burials in Roskilde Cathedral gives a complete statement of the skulls and of many long bones, with restorations of the living profile. How long will it be before we may have as much done in Westminster Abbey? This line of inquiry has been carried further in (5), the paper on the reconstruction of features of historic persons from the skull, comparing skull, death mask, bust and portraits, from Schiller, Raphael, Kant, Bach, Swedenborg and others. Similar comparisons in Egypt may be seen in my *Diospolis*, xlvi, and *Ancient Egypt*, 1922, 79.

In all the above works the Danish pre-eminence in producing fine plates is fully maintained.

FLINDERS PETRIE.

Europe: Ethnography.

Rütimeyer.

Ur-Ethnographie der Schweiz. By L. Rütimeyer: Schweizerische Gesellschaft für Volkskunde. Basel, 1924.

62

The collation of archaic apparatus, the use of which still survives among the local peasantry, with similar objects that have been recovered by archæologists from prehistoric deposits, is a novel and fruitful departure in ethnography. The present volume is an illuminating contribution to such study; for both classes of antiquities are well represented in Switzerland. A series of archaic objects, the use of which has survived till recent times, is here described and illustrated; parallels are cited from other European and extra-European lands, and an attempt is made to trace these survivals to their roots in prehistoric times. Particularly instructive is the treatment of stone lamps often quite like palæolithic specimens, birch-bark candles traceable back to the neolithic lake-dwellings, and toy cows compared with the well-known clay models from Schipenitz and contemporary stations. The collection will be a great help to the archæologist in his task of interpretation and a boon to the comparative anthropologist. Dr. Rutimeyer's series admirably illustrate the phenomena of convergence and divergence and a study of them is to be recommended, particularly to those who are inclined to infer direct contact from existing similarities of customs and artifacts.

V. G. C.

Europe: Archæology.

Schmidt.

Vorgeschichte Europas. (Band I.) By Hubert Schmidt. Leipzig and Berlin: Teubner, 1924. (Volume 571 of this publisher's *Aus Natur- und Geisteswelt.*) Pp. 103. Price 1 mark 20.

63

A comprehensive survey of European prehistory by Professor Hubert Schmidt, even in a cheap and popular form, must command general attention; the author is already well known even in this country through his admirable catalogue of the Schliemann Collection, his skillful conduct of the archæological department of the Pampelly expedition to Turkestan and his epoch-making excavations at Cucuteni. This little book will, indeed, prove an immense boon to all students whose purview is not limited by the Alps and the Rhine. Not only is it incredibly cheap, but it is a veritable mine of information on the archæology of Central and Eastern Europe, illustrated by a judiciously selected (but unfortunately very badly arranged) series of type-objects and supplemented by useful notes on Turkestan, China and Cappadocia. I do not suppose that the views here advanced as to the relations between the Continent and the Aegean, or the chronology on which they are based,

will win general acceptance outside Germany. This clear and lucid statement of the Germanist thesis is none the less valuable. And, even if they cannot bear the interpretation here placed upon them, the facts adduced are abundant and generally indisputable. Among the rare exceptions to the last statement I may cite the attribution of the perforated stone battle-axes found in Britain to the long barrow culture. No guide to the Stone and Bronze Ages comparable in price or erudition is at present on the market.

V. G. C.

Sociology.

Buxton.

Primitive Labour. By L. H. Dudley Buxton, Lecturer in Physical Anthropology in the University of Oxford and formerly Albert Kahn Travelling Fellow. Methuen & Co., Ltd. London. 1924. Pp. viii + 272. Price 7s. 6d. net.

64

In this little work our Fellow, Mr. Dudley Buxton, has set out to describe the different types of work carried on by various kinds of primitive peoples, in very many cases as the result of his own observations made during visits paid to their countries. He has been at great pains to distinguish the duties allocated to either sex in the communities which he describes, and to show at still greater length the kinds of work which are involved by residence under varying geographical and climatic conditions.

The descriptions of the types of environment have been given very fully and with considerable skill, though we missed an account of the park-lands which are found distributed between the forests and steppe-lands of South Russia. One would have liked, too, to have been told more of the conditions which cause open spaces to occur in deciduous forests.

There is a chapter on prehistoric man and some pages in the final chapter are devoted to the same subject. In the latter we were surprised to see the statement that, with the exception of one skeleton from Chancelade, all those known to date from Magdalenian times belong to the Cromagnon race.

The volume gives a useful summary of the conditions of primitive labour, and will be of great service to the beginner and to the general reader. H. J. E. P.

CORRESPONDENCE.

Britain: Archæology.

Garrod.

*To the Editor of MAN.***Solutrean Implements: a Correction.**

SIR,—My note on Solutrean implements (MAN, 1925, 49) contains three misprints, which affect the sense. I should be much obliged if you would kindly publish the following correction:—

65

P. 84, last line but one, "isolated" should read "undated."

P. 84, last line, "formation" should read "patination."

P. 85, line 3, "entirely" should read "certainly."

D. A. E. GARROD.

Britain: Archæology.

Reid Moir.

*To the Editor of MAN.***Solutrean Implements in England.**

66

SIR,—Though it is evident that Miss Garrod (MAN, 1925, 49) and Mr. Burkitt (MAN, 1925, 47) do not quite see eye to eye upon the question as to whether, or not, certain flint implements found in England are of Solutrean age, yet they are in agreement in claiming that the well-known "laurel-leaf" blade is not typical of this period.

Now, this is an important question, and, while I do not wish to assert that their conclusion is incorrect, I nevertheless feel it to be necessary to ask them for more precise evidence upon this matter.

It seems to me that, to establish the correctness of their conclusion they must—

(a) define clearly what is meant by the term "typical laurel leaf," as applied to a flint implement, by describing its exact form—the manner in which it is made—the peculiarities of the flake-scars it exhibits—and whether it shows—or does not show—retouching along its edges.

(b) prove that other specimens, of which the form, and method of manufacture, are indistinguishable from that of undoubtedly Solutrean blades, have been found in deposits which are, without question, either earlier, or later, than this cultural horizon.

(c) give the names, and other details, of the sites where such discoveries have been made, and put forward conclusive evidence to show that the Solutrean-like specimens there found are of the same age as the mass of the other artefacts discovered with them.

(d) give full references to such discoveries, if an account of them has been published.

If Miss Garrod and Mr. Burkitt will provide the evidence asked for above, no one, I take it, will ever again dispute their contention that a "laurel leaf" blade is not typical of the Solutrean period.

Yours faithfully,

J. REID MOIR.

Stonehenge.

Stone.

To the Editor of MAN.

Stonehenge: A Reply.

SIR,—The review on the writer's work on Stonehenge, by Admiral Boyle T. Somerville, published in MAN, 1925, 35, appears to have been written in some respects without sufficient consideration of well-known facts.

67

On page 2 of the writer's book on Stonehenge is a passage as follows:—

"The present structure of Stonehenge, as we now see it, is all of one period. The erection of the sarsens is contemporaneous with that of the blue stones, the work having been continuous."

Admiral Somerville quotes this passage and supposes it to be inconsistent with another passage (which he quotes from page 65) regarding the previous erection in the Aubrey Holes of an *older* circle formed of the blue stones which had been brought from Wales. He has entirely overlooked the fact that the remarks on page 2 are specifically made in reference only to the stones as arranged in *the present structure*; and he appears to suppose that the blue stone circle of the present structure was the same thing as the problematical blue stone circle which may at an earlier period have occupied the "Aubrey Holes."

In a previous paper in MAN, in September, 1922 (No. 77), Admiral Somerville assumes that, in the existing structure of Stonehenge, the outer circle of sarsens and the inner circle of blue stones were erected at two different dates, and asks:—"Which of these two entirely distinct monuments is that for which a date for "erection is sought?" This mistake was corrected by the present writer in MAN for November, 1922 (No. 98), but is, nevertheless, repeated in the review now under consideration.

Other remarks in the review based on this misunderstanding need no further comment.

That the *present* structure of Stonehenge—both sarsens and blue stones—is all of one period is fully established by the results of the excavations conducted by Professor Gowland in 1901 and by Colonel Hawley in 1920-21. This is one of the most important facts ascertained by these excavations.

In the chapter entitled "The Age of Stonehenge," in the writer's work under review, the date there discussed is, of course, the date of the *present* structure. The dates of previous works which may have existed in earlier times on the same site do not form any part of the subject-matter of the book. For a consideration of the dates of these previous works we must await the results of the important excavations now being undertaken by Colonel Hawley.

In reference to this matter, Admiral Somerville remarks that the work on Stonehenge under review "has appeared too soon." Another reviewer says:—"It is just the book that is wanted, also the present is just the time when it is 'wanted most.' It is proverbially difficult to please everybody."

When Colonel Hawley has completed his investigations we shall look forward to a complete account of his remarkable discoveries in connection with earlier works on the Stonehenge site. The present writer ventures to hope that his own work dealing with the existing structure may then still be found useful for reference as a companion volume.

In regard to the method of erecting the stones, as suggested in Chapter VI, Admiral Somerville asks:—"What has happened to the vast mounds of earth " and chalk, etc., which must have been brought to the site for the purpose?"

This can scarcely be considered a practical difficulty. The mounds were of no great size—only 13 feet in height—and the same material would have been used over and over again in successive positions. If dug from near by, the earth, when the work was completed, might have been returned to the holes from which it had been dug. Or if deposited here and there in spoil heaps, might have been utilised later on in the formation of some of the numerous Round Barrows in the vicinity.

E. HERBERT STONE.

Ethnology.

Thomas.

To the Editor of MAN.

Independent Invention.

68

DEAR SIR,—Professor Sir William Ridgeway's note (MAN, 1925, 52) on Mr. Henry Balfour's paper on Thorn Traps reminds me of a certain correlative instance.

As a child in Ceylon at a small up-country school, I was much interested in the paper and bamboo contrivances the natives hung or fixed outside their *boutiques* in the bazaars to revolve in the wind.

A Japanese bamboo paper knife hung by a string through the tassel hole, revolved merrily, I found, until the torsion of the string became too strong for it.

We were all devoted to slinging at this time: a pastime to which a native servant had introduced us.

The first-mentioned interest led me to tie a string to my paper knife: the second, I feel, prompted me to whirl it round in the air.

At any rate I did so and the result astonished me. I was the introducer of a toy which became very popular for a time, and which I only learned some 15 years later was the *bull roarer*.

Yours, etc.,

ERNEST S. THOMAS.



FIG. 2.—RETURNED FROM FATTENING.

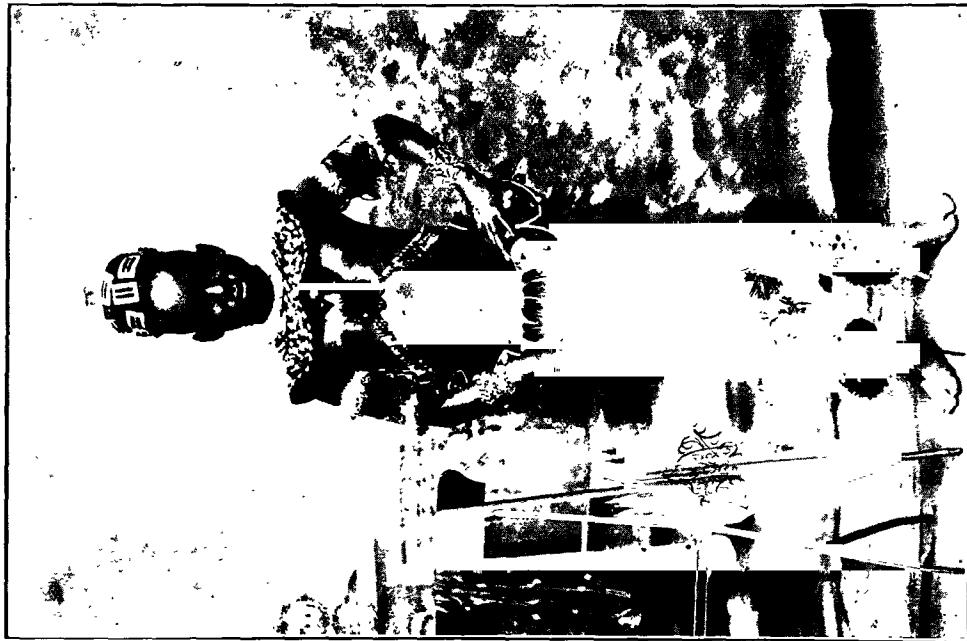


FIG. 1.—A BETROTHED GIRL GOING TO THE FATTENING.

SECLUSION OF GIRLS AMONG THE ELIK, OLD CALABAR.

ORIGINAL ARTICLES.

With Plate H.

Africa, West: Sociology.

Malcolm.

Note on the Seclusion of Girls among the Efik at Old Calabar.

69

By L. W. G. Malcolm.

The material contained in the present communication is concerned with the seclusion of girls of the Efik tribe, at Old Calabar, Southern Nigeria, and was obtained during a stay in that town in 1918. For assistance, also for permission to consult his notes, I have to thank the Rev. J. K. McGregor.

As in Ekoiland, the girls of the Efik are secluded in huts for a shorter or longer period. There appears to be some social significance in the length of the period she is secluded, as only free-born girls of well-to-do parents are able to conform to the custom. The length of time a girl is able to spend in the fattening-hut is an indication of her parents' prosperity. As she is betrothed before being placed in the seclusion hut, it also has an influence in determining her social position as a wife, as no well-born man of the tribe would marry a girl who has not been secluded for a shorter or longer period.

After reaching the age of puberty the girl (*n-kaiferi*), is clothed in an embroidered cloth cap, a loin-roll of bright coloured cloth, a collarette ornamented with beads and cowry shells, beaded shoulder braces and leglets of gaily-coloured cloth or coiled brass rods. Necklets and armlets of beadwork may also be worn (Fig. 1., Pl. H.). She is then taken to the fattening-hut (*m-bobi*) by her mother. The huts are situated on the outskirts of the village. Her period of seclusion (*okukhö m-bobi*) may extend from six months to two years according to her parents' financial position. Whilst in the hut she is called a woman of seclusion (*n-wan ñ-kukhö*). During the fattening process she is compelled to eat vast quantities of fat-producing foods, including pounded yams (*fu-fu*) cooked in palm-oil. She is not allowed to exert herself in any way, and on no account must she perspire. Her face and body are not washed, and she is smeared with clay. White cloths are tied round her neck, wrists and ankles to prevent "evil spirits" retarding the process. Whilst she is in *ufök ñ-kukhö* she is not allowed to touch anything in the hut, and she must avoid all possible contact with the ground. When she has occasion to leave the hut and go into the yard of the compound she calls out "*Ö nukhö mi, nukhö fi o.*"*



FIG. 1.—A WEDDING GROUP.

* Lit. "You that spy on me, I spy on you."

At or near the end of her period of seclusion she undergoes the operation of clitoridectomy (*ana m-bobi*), usually at the hands of her mother. A piece of coconut shell is shaved very thin and a hole bored through. The glans clitoridis is drawn through this hole and cut off with a sharp knife or splinter of glass. It is believed that the operation has the effect of making the girl sterile. Notwithstanding this, the women themselves are the chief supporters of the custom and resent any interference.

At the time when her period of seclusion is over she is taken from the hut (*öwöro m-bobi*) and appears before the members of the tribe, who greet her with great rejoicing and who offer her presents. She is attired in a loin-cloth, bead-braces, and numerous neck, arm, and leg ornaments. Her hair is dressed in the form of three crowns, and is ornamented with beads, feathers, etc.

As she leaves the hut she dances before the public, and sings,

" Ñ-wan ete atañ idet ita
Se ama, se ama, anam, se ama anam
Idet ita ösöñ odot enye
Se ama, se ama, anam, se ama anam
Ete esie onyuñ ama enye
Se ama, se ama, anam, se ama anam."

This song refers to the custom of dressing the hair in the form of three crowns, which can only be worn by the girls who have been in the fattening-hut. (Pl. H., Fig. 2.)

Before she is claimed by her bridegroom she is taken to the *isa ekpo* of her ancestors. A white chicken is suspended from a cord round her neck, and when she leaves it is dropped to the ground in front of the *isa ekpo*.

The marriage ceremony is closely connected with the *ekpo* (ghost) cult of her tribe, and she must certify on oath before the *isa ekpo* that she will be faithful to her husband. The form of oath varies among the Cross River tribes, but in all sacrifices must be made to the *ekpo*. The violation of the oath would mean that the *ekpo* would either cause the woman to bear stillborn or deformed children, or make her become sterile.

L. W. G. MALCOLM.

Sociology.

Seligman.

Cross-Cousin Marriage. *By Brenda Z. Seligman.*

70

In so far as any theory encourages the careful observation of facts and helps in the understanding of these, that theory is useful. From this point of view the *ntoro* hypothesis of cross-cousin marriage put forward by Captain Rattray and Mr. Dudley Buxton in the *Journal of the African Society* (Vol. XXIV, No. XCIV) is of peculiar interest, as Captain Rattray has returned to Ashanti and will be able again to examine his data and their implications.

Briefly stated the hypothesis is that cross-cousin marriage is founded on the co-existence of two forms of descent in one social system. In Ashanti, clan descent is matrilineal, but in addition to the clan there is a grouping by divisions called *ntoro* which descend in the male line. Reincarnation is believed to take place in alternate generations, and the authors suggest that it is necessary for a spirit to be re-born into the clan and the *ntoro* of its previous life, and in order to bring this about cross-cousin marriage is necessary.

Underlying the *ntoro* hypothesis, there is a principle of the first importance, namely, that of the correlation of beliefs and social organization. Whatever may be the ultimate fate of the hypothesis attention to this principle cannot but stimulate the observer.

Besides reincarnation, another idea, not mentioned by the authors but possibly often associated with reincarnation, may be of considerable importance in relation

to kinship ties—that is the desire to leave offspring who, by right of birth, are capable of carrying out the ritual offerings necessary for the well-being of the departed.

Three questions may be asked:—

(a) Does the *ntoro* hypothesis give a reason for the dominance of the cross-cousin type of marriage?

(b) Can it account for the origin of this type of marriage?

(c) Ignoring, for the present, conditions in other parts of the world, how far does the *ntoro* hypothesis correspond with the facts in Ashanti?

It will be convenient to examine the third question first.

In their theory of cross-cousin marriage the authors begin by supposing that there are two intermarrying matrilineal exogamous groups and two patrilineal exogamous *ntoro* divisions, furthermore that a brother and sister marry a sister and brother. That is to say two men who are cross-cousins exchange sisters as wives.

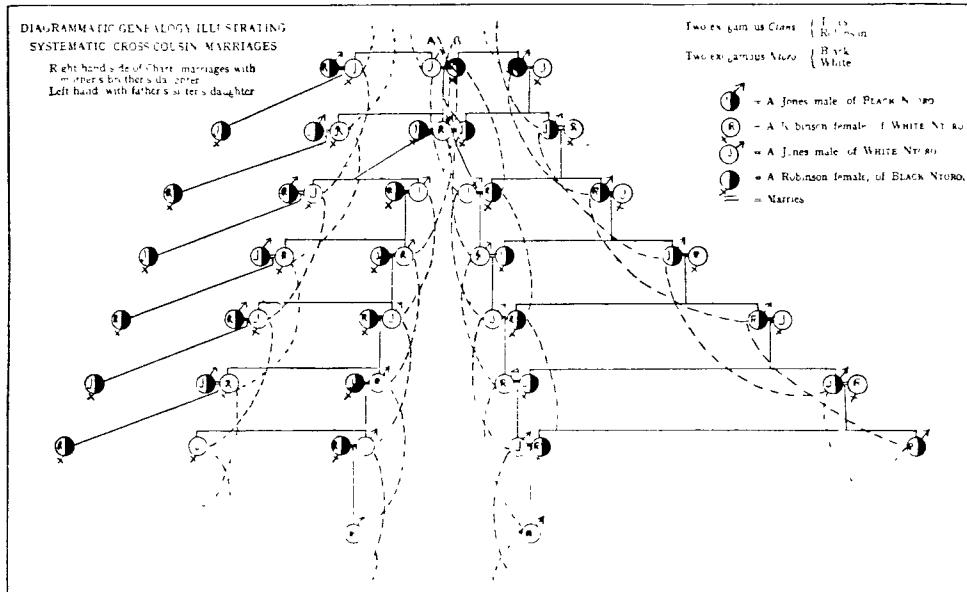


FIG. 1.

They then discover that such an arrangement accounts for the following rules which they state "we already know to exist."

" (1) In each alternate generation we get children of the same clan *plus* the same *ntoro*.

" (2) In the case of males, they are of the same clan and the same *ntoro* as the paternal grandfather; in the case of the females, of the same clan and *ntoro* as the maternal grandmother.

" (3) In each alternate generation the clans change, so that each clan successively gains units.

" (4) Down the male line, the original male *ntoro* persists, that of the female, the wife, being lost.

" (5) As soon as descent passes into the female line, however, the lost *ntoro* is regained and, other things being equal, balances exactly *ntoro* for *ntoro* just as we see clan balancing clan."

(1) This is certainly so in Captain Rattray's table, reproduced in fig. 1, but is it so among the Ashanti? We know from Captain Rattray's book* that the Ashanti

* *Ashanti*, 1923.

have a number of clans and *ntoro*, that all are exogamous and that cross-cousin marriage is customary. But so far we have had no indication that either the clans or the *ntoro* are arranged in any definite intermarrying groups and we must await genealogical evidence before we can suppose that this may be so. Further, ordinary cross-cousin marriage need not cause the *ntoro* and clan to recombine every alternate generation, and in fact it could not do so, unless (a) the clans and *ntoro* were actually both limited to two divisions, or (b) in every case a brother and sister marry a sister and a brother; (c) and that marriage with the father's sister's daughter be enforced, but marriage with the mother's brother's daughter be forbidden.

(2) This appears to be a striking fact, but actually males and females inherit in exactly the same way, both men and women are of the same clan and *ntoro* as their paternal grandfather and their maternal grandmother, because these two people

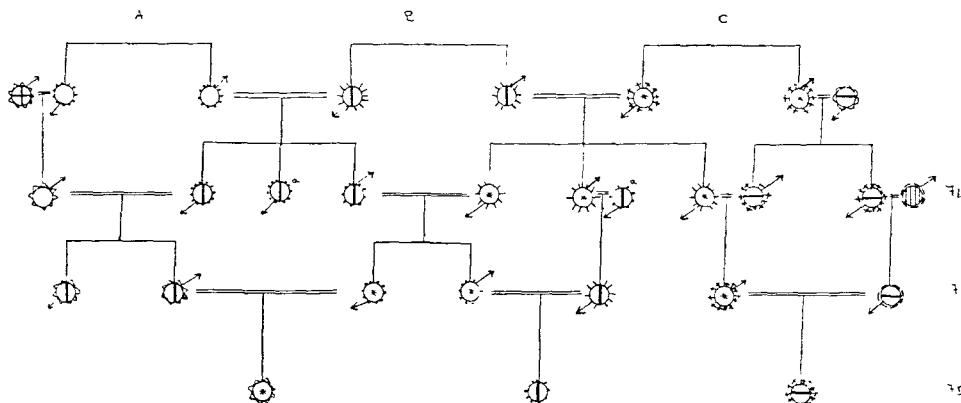


FIG. 2.

The signs in the body of each circle represent clans.

The signs on the edge of each circle represent *ntoro*.

An indefinite number of clans and *ntoro* have been supposed.

The male descendants of family A have in each generation married the daughter of the mother's brother and the *ntoro* and clan of A have not reappeared together in subsequent generations.

In fig. 1 the son of B and C families has married the daughter of his father's sister, so in fig. 2 the clan and *ntoro* of the male B has reappeared.

The son of male C has married the daughter of his father's sister, so that in fig. 2 the clan and *ntoro* of C have reappeared. The son's son of male C has also married the daughter of his father's sister and in fig. 3 the clan and *ntoro* of fig. 1 have again appeared, thus giving the alternation of generations seen in Captain Rattray's diagram. It is seen here that this alternation of clan and *ntoro* can only take place by means of the marriage with the daughter of the father's sister—not cross-cousin marriage—when an indefinite number of clans and *ntoro* are involved.

are brother and sister. This is so in the table (fig. 1) because the conditions are those described above under (b). If the conditions described under (a) were prevalent, the paternal grandfather and the maternal grandmother would be of the same clan and division, though not true brother and sister. But this would not be so in ordinary cross-cousin marriage where there are more than two clans and marriage with both the cross-cousins is allowed (fig. 2). However, if a man marries his father's sister's daughter the son who bears his father's *ntoro* reintroduces this into his father's clan to which he himself naturally does not belong, and so his son and daughter will belong to the same clan and bear the same *ntoro* as their paternal grandfather and their maternal grandmother. If on the contrary a man marries his mother's brother's daughter he distributes his *ntoro* into other clans. If, as supposed (contrary to the Ashanti evidence), there are only two exogamous clans, then, of course, he can only introduce his *ntoro* into the clan of his father, and the

recombination of clan and *ntoro* in alternate generations will occur as it must with the marriage of a man with his father's sister's daughter. But should there be an indefinite number of clans and *ntoro*, as there actually is among the Ashanti, this alternation of clan and *ntoro* and recombination in alternate generations can only take place through marriage with the daughter of the father's sister. We want further genealogical evidence to show that when a child is called after his or her grandparents it is considered necessary that the *ntoro* and clan should be the same, also we need to know which kind of cousin marriage predominates.

(3) The facts concerned here have been dealt with above, but it must be born in mind that the clan is only reckoned in the female line, it never either gains or loses units, the children of all its *women* are its *members* from generation to generation.

(4) This is simply a statement of patrilineal descent, which we know the *ntoro* follows.

(5) Having presupposed a dual organisation there must naturally be recombination of units every succeeding generation.

In paragraphs 3, 4 and 5 there is a certain confusion of ideas from which it would appear that similar units had two separate modes of behaviour. Actually the clan is a social unit of great importance; the *ntoro* is a division which recognizes the male hereditary principle, and is mainly important on account of religious ideas. The balancing of the two divisions would be necessary only if both had social importance. Until some evidence can be brought forward that suggests this, there can be no reason to suppose that the native feels any loss to his clan by reason of female descent. Although kinship is bilateral, clan descent is inevitably unilateral; it is impossible to belong to the clan of both parents, therefore the question of the line in which descent is reckoned becomes of no consequence. Only because we are accustomed to patrilineal descent do we suppose that with matrilineal descent a man's children are *lost* to his clan. A man accustomed to matrilineal descent sees his clan continue in the children (male and female, *nota bene*) of his sisters, and so long as women are born in the clan the line cannot die out. He might equally bewail the fate of a woman in patrilineal society whose children are *lost* to her clan, and the danger that would befall a man in such a society who had daughters and no sons.

Should Captain Rattray, however, find that *ntoro* divisions have social functions, that *ntoro* brotherhood corresponds in some way to clan brotherhood with its special privileges and duties, the idea of balancing the numerical strength of these groups might be important. It might then be conceivable that an Ashanti would consider the question of loss to his group. But such evidence would deserve the closest scrutiny, for conflicting ideas of clan descent might have arisen in comparatively recent times, from culture contact with a people possessing a different form of descent, and need not date back to an ancient and hypothetical dual organisation.

We can now return to the important point of reincarnation. Captain Rattray states:—

“ Yet again, unless we understand the full significance underlying that aspect of Ashanti religion which enjoins that the spirits and memories of famous ancestors be venerated and propitiated, we cannot fully grasp what a calamity in the Ashanti mind the extinction of his clan entails.

“ For not only are human beings divided into exogamous clans and *ntoro*, but in the spirit world (*samando*) the ghosts continue to be concerned with and able not only to confer good upon, but to receive benefits from, those members of the human community alone who were their clansmen upon earth. *I believe also it may yet be shown* that the only hope the inhabitants of the ‘ cold

" ' shadowless spirit world ' have of reincarnation upon ' the warm sun-bathed " ' earth ' lies in being born again into that *abusua* (*and just possibly* also *ntoro*) " of which they were members upon earth. The extinction of the clan would, " therefore, mean the extinction of all hope of return to this world."*

Captain Rattray has not yet given us the evidence which has changed his supposition into the assumption that reincarnation into the same *ntoro* and the same clan may be equally important for the spirits. We may still hope for this evidence, but until we have it, the main argument for the hypothesis remains in doubt.

We may now return to our second question : Can the theory account for the origin of cross-cousin marriage ? Taking into account the author's proviso that the dual organization must be presupposed, does this hypothesis of joint male and female descent account for the origin of cross-cousin marriage ?

The matrilineal and patrilineal descent of clan and *ntoro* respectively account for the prohibition of marriage between the children of two brothers as well as the marriage between the children of two sisters. This also renders the marriage with either the brother's daughter and the sister's daughter or the father's sister and the mother's sister impossible. The possibility of marriage between children of brother and sister remains, but its origin is still obscure.

It has already been pointed out (*a*) that the five conditions put forward by the authors would be fulfilled whenever the two exogamous units were each limited strictly to two. But this would only enforce cross-cousin marriage in a "classificatory sense." It would imply no compulsion to true cross-cousin marriage.

In order to understand cross-cousin marriage we must ascertain why the children of actual brother and sister should marry, as well as why the children of two brothers and of two sisters should never marry. In other words the problem of cross-cousin marriage is one in which full weight must be given to the family as well as the clan and *ntoro*.

The authors further suggest that the reincarnation theory would be a reason for the prohibition of marriage between grandfather and grandchild. But precisely where this belief is prevalent marriage between relations separated by two generations is allowed, and this type of marriage is closely associated with cross-cousin marriage. Elsewhere I have put forward the view that the belief in reincarnation has facilitated the marriage between persons separated by two generations.†

We may now put our first question :—

Does the *ntoro* hypothesis give a reason for cross-cousin marriage ? So far as I am able to understand the hypothesis of Captain Rattray and Mr. Dudley Buxton, the cross-cousin marriage is regarded as the outcome of certain beliefs which are supposed to be prevalent among the Ashanti.

(1) A theory of conception and heredity by which the embryo is formed by the generative fluid of the male and the blood of the female. Only the blood of the mother can be inherited, but apparently there is no "spirit" associated with the blood; on the other hand there is an element associated with the male generative fluid which survives death.

(2) This "spirit" which has survived death is impatient to be reborn, but so close is its connection with its previous incarnation that it can only be reborn in a person of the same clan and *ntoro*.

A dual organization with exogamy *being already in existence* it was discovered by the Ashanti that cross-cousin marriage brought about this combination of clan

* *Ashanti*, pp. 79-80. The italics are mine.

† "Marital Gerontocracy in Africa," *J.R.A.I.*, Vol. LIV, p. 246.

and *ntoro* every alternate generation. Satisfaction was thereby gained for the spirits and a stable organization for men by which clan and *ntoro* were balanced.

The authors state :—" In regard to ultimate origins we are not putting forward any hypothesis. The dual organisation must be presupposed." This is unfortunate, for at present definite evidence for dual organization in Africa is wanting; it is also difficult to understand how, given the organization, the motive for it can be supposed to come later. Had the authors a firmer belief in the ideas they have formulated for the Ashanti, they must have seen that these in themselves would be sufficient foundation for a dual organization.

The authors thus suggest a spiritual and a material reason for the cross-cousin marriage. On the spiritual side the evidence is as yet slender, but if it should be confirmed that a man's spirit *can* only be reincarnated in the same clan and *ntoro* as himself, and a man *must* be named after his paternal grandparent, then there is a powerful reason for marrying the father's sister's daughter, but none at all for marrying the mother's brother's daughter, unless there is a dual grouping both of clan and *ntoro* in operation. But if there is already in existence a dual system of clans and *ntoro* there is no need for a man to marry his true cross-cousin, for any woman in the correct group would bring clan and *ntoro* together again.

On the material side, the reason for cross-cousin marriage suggested, is the balancing of *ntoro* and clan numerically. I have already shown that this rests upon an assumption which would only occur to one accustomed to patrilineal society, but before rejecting entirely the idea of balancing clan and *ntoro* the matter must be looked at from another point of view. Can the *ntoro* be regarded as degenerate totem clans belonging to a patrilineal people who have fused with a people organised on a matrilineal clan basis to form the Ashanti nation? The possibility is by no means negligible, but this puts the problem in an entirely different perspective and cannot be discussed here.

At this point comparison with the Nyanja may be helpful, although the evidence is incomplete and somewhat contradictory. The Nyanja on the eastern shore of Lake Nyasa have two forms of descent. Clan organization and succession are matrilineal, but there is a relationship traced through the father, called *chilawa*, which appears to correspond to a surname. Marriage between both types of ortho-cousins is forbidden, and though the Rev. H. Barnes does not actually state that cross-cousin marriage is permitted, the existence of this type of marriage is assumed in his paper. An indication of the importance of the paternal relationship is seen from the following statement :—" The *chilawa* name, too, is the one that " a man repeats with a short thanksgiving when he has sneezed, the idea being " that a sneeze is an indication of a blessing from the ancestral spirit on the " father's side."*

The Nyanja are neighbours of the Angoni and the Yao; among the former descent is patrilineal, while the latter are matrilineal. Miss Werner suggests that the *chilawa* system may be borrowed from the Zulu, and the southern tribes themselves call it the Angoni system. There is no trace of the *chilawa* among the western and southern Nyanja, but an incident related by Miss Werner is significant. On a certain occasion, when the spirit of a dead man needed propitiation, it was the son, not the sister's son, that was called upon to make ceremonial offerings. Miss Werner states that whereas the marriage with the daughters of the father's sister is allowed among the Nyanja, it is forbidden among those who recognise the *chilawa* relationship.† Among the Nyanja the evidence for culture contact offered by the double form of descent is too obvious to be ignored; at the same time the association of the spirit world with the patrilineal,

* " Marriage of Cousins in Nyasaland," MAN, 85, October 1922.

† " British Central Africa," pp. 47, 253, 254.

and the world of affairs with the matrilineal line corresponds in a striking manner to the clan and *ntoro* system of the Ashanti. However, the very marriage which would bring about a recombination of the two lines in the second generation for the purpose of reincarnation is actually forbidden.

It would seem a truism to state that cross-cousin marriage implies the recognition of both patrilineal and matrilineal kinship. Both Mr. Hocart and Professor Radcliffe-Brown have drawn attention to the part played in ritual by the maternal relatives in patrilineal societies.* Among the Ashanti and the Nyanja the position is exactly reversed, and this reversal cannot be accidental. The question is too complicated to be discussed here, but I would like to draw attention both to the "fixed patterns of behaviour for each recognized kind of "kinship relation" stipulated by Professor Radcliffe-Brown, and the psychological attitude towards the maternal and paternal relatives in matrilineal and patrilineal society, discussed with so much insight, though from rather different angles, by Dr. Malinowski and Dr. Ernest Jones.† Neither these points of view nor the possibility of contact or fusions can be ignored in the clan and *ntoro* problem of the Ashanti.

It will be well to re-state the facts.

In Ashanti every man and woman inherits two elements, one from each parent, but a man can only transmit one element to his children (whether male or female) and a woman another to her children of both sexes. The man transmits his *ntoro* and the woman her *abusua*, blood; the Ashanti believe that a man is incapable of transmitting his blood. Here we have a simple physiological conception. As the clan is thought of as a consanguineous body a man belongs to it by reason of his mother's blood, his father's blood is not represented in his clan at all nor will his clan be of importance to his son. Hence a man may marry into his father's clan. All a man's social obligations are to his clan. The *ntoro* is transmitted from the father to his children, but implies no social obligation except exogamy, though many religious and possibly totemic duties; and just as a man cannot inherit his mother's *ntoro*, so he can marry into his mother's *ntoro* division.

Granting that the desire to live again is natural, and that it is also natural to desire to live again in similar conditions, it would seem that a man would want to be reincarnated in his old clan. We know that so-called "primitive people" have no difficulty in accepting the idea of reincarnation, either into human beings (sex is frequently immaterial), or animals. Numerous examples from Africa could be quoted, but space forbids. Methods by which spirits become reincarnated are not easy to ascertain, but most of those described ignore the necessity of physical paternity. As a rule the spirit to be reborn enters the womb of a woman, and when the child is born it is recognised as a reincarnation of some relative. The Ashanti apparently are more particular; not only do they definitely recognize paternity, for *ntoro* also means *semen*, but the spirit of a dead man requires to be reanimated in the womb of the sister's daughter by his own son. But perhaps the Ashanti has doubts of the actual physical descent of the spirit, for the naming ceremony quoted by Captain Rattray can only be regarded as symbolising paternity.‡

* Hocart, MAN, 4, (1923). 96 (1914). 132 (1924). A. Radcliffe-Brown: "The Mother's Brother in S. Africa," *South African Journal of Science*, 1924.

† Malinowski: *Psyche*, Vol. IV, No. 2, 4; Vol. V, No. 3. Ernest Jones: "Mother-Right and the Sexual Ignorance of Savages," *International Journal of Psycho-Analysis*, Vol. VI, Part 2, 1925.

‡ Captain Rattray's words are: "that at the naming of a child, e.g., after his grandfather, the infant is taken to him on the eighth day, and he spits in his grandchild's mouth to strengthen the spirit already there, which is, of course, his own *ntoro* passed on

Certainly such material ideas of reincarnation would be a reason for the marriage with the daughter of the father's sister (not cross-cousin marriage), though it may be questioned whether such ideas are sufficiently primitive and general to form a basis of so widely spread a custom as the cross-cousin marriage.

In conclusion I would say that the discovery of the *ntoro* among the Ashanti is of great importance and may lead to further discoveries elsewhere, but in looking upon this form of descent as the basis of cross-cousin marriage, not only has some violence been done to the evidence, but the whole problem has been short circuited. Further, it is not necessary to suppose that wherever two forms of descent are found in operation among a single people there is evidence for a dual organisation.

BRENDA Z. SELIGMAN.

America, Central: Archaeology.

Thompson.

The Meaning of the Mayan Months. By J. E. S. Thompson.

A great deal of attention has been paid recently by the different schools of anthropology to the correlation of Mayan and Christian dating. A few months ago Mr. Herbert J. Spinden, of the Peabody Museum, produced a new scheme,* in which he claimed to correlate the two systems to the very day, making O POP, the Mayan New Year Day, fall on the winter Solstice on the date, which he states was arbitrarily chosen to initiate the calendar namely 7—1—13—15—12. 7 EB O POP. However, the Mayan months have quite straightforward translatable names, which seem to contradict his theory, and any suggestion that the names of the months changed through the tardation due to the lack of intercalated days is ruled out by the phonetic character of several of the glyphs.

However, to understand the question thoroughly it is necessary to make a brief survey of the annual activities of the Maya. Although data on this subject is lamentably absent for the pre-columbian period, the modern primitive Maya can give us a very clear idea of the agricultural routine of his ancestors; there is no reason to believe that there has been any drastic alteration in the last few centuries, and we can safely assume that agriculture has continued for some hundreds of years on the same lines in Yucatan. Diego Landa, the great authority on the Mayans, only touches very briefly on their civil occupations, but his few sentences amply confirm the innate conservatism of the Maya peasant, linking up the pre-Spanish system with that employed at the present time.

In December or January the virgin bush is cut down, only the big trees of hard wood being left untouched; the neighbours assist one another at this work. The scrub is left to dry until May, when it is burnt, whereupon the Mayans leave their villages and take up residence on their "Milpas" (allotments). Then come the heavy late Spring rains, softening the ground, which has been parched by the preceding winter drought. Armed with sharp pointed sticks and bags of maize they cross their fields, digging holes with their sticks, into which they drop three or four maize seeds. About the end of September or the beginning of October the corn begins to ripen, and then each cob is twisted downwards and left hanging for several days until it finally ripens, when the crop is gathered in. The object of this operation is to prevent rain entering and rotting the seed.

Every one of these events is faithfully indicated in the calendar. Landa, in his "Relacion de las Cosas de Yucatan," places the Mayan year side by side with

"through his son (not daughter, be it noted, for a grandchild by a daughter would not be of the grandfather's *ntoro*). The law is irrevocable which insists that only one of like *ntoro* can perform this ceremony" (Ashanti, p. 54). This would appear to be an example of what psycho-analysts call "overdetermination."

* "The Reduction of Mayan Dates." H. J. Spinden, Peabody Museum, 1924.

the Christian year 1553-1554. The only alteration I have made is to add ten days to bring the Julian calendar into line with the Gregorian.

Name of Month.	Starting Day in Gregorian equivalent.	Revised Spelling.	Meaning of word in English.
1 POP	July 26	POP	Maize leaves.
2 UO	Aug. 15	UOL	Cobs.
3 ZIP	Sept. 4	ZIP	Ripening of the fruit.
4 ZOTZ	Sept. 24	Zotz	Lazing on the ground.
5 Tzec	Oct. 14	Tzeh	Husking.
6 XUL	Nov. 3	XUL	The end.
7 Yaxkin	Nov. 23	Yaxkin	The fresh days.
8 MOL	Dec. 12	MOL	To collect together.
9 CHEN	Jan. 1	Chen	Clean.
10 Yax	Jan. 21	YAX	Green.
11 ZAC	Feb. 10	ZAC	White.
12 Ceh	March 2	CEL	Roast.
13 MAC	March 22	MAC	Crust
14 KanKin	April 11	Kankin	Yellow days.
15 MUAN	May 1	MUAN	Showery.
16 PAX	May 21	PACH	Take up land.
17 Kayab	June 10	Kaanyaab	A lot of heavy rain.
18 CUMHU	June 30	CUMHOL(AH)	To make holes in something soft.
19 UAYEB	July 20	UAYEB	Unlucky and end of work.

YAXKIN (Nov.-Dec.) the beginning of the new agricultural year. Yaxkin means New Days.

MOL corresponds to the end of December, when the brushwood was cut down and collected. MOL means to collect or pile together.

CHEN, the following month, means clean—that is the fields are now bare and empty.

YAX means green or beginning—but I must confess I have been unable to fit in this meaning.

ZAC is the word employed for the measure used to measure out the new “Milpas.”

CEH has lost the letter L. It should be CEL—roasted—probably referring to either the burning of the brushwood or to the condition of the soil after the drought.

MAC. Crust, possibly referring to the thick hard crust formed by the drought.

KANKIN (April) is the last month of the dry season, and means yellow days, referring to the parched up condition of the country.

MUAN (May) is the month when the first light rains fall. The word means showery.

PAX (May-June) is a corruption of PACH—meaning to take possession of a place, referring to the removal from the village to the “Milpas.”

KAYAB (end of June) the period of the heavy downfalls. KAYAB is a corruption of KAAN YAB meaning a lot of heavy rain.

CUMHU (July) the sowing season. CUMHU is a corruption of CUM, soft, and HOLAH, to make holes, that is digging the sowing sticks into the soft earth.

UAYEB (July 20th) the five unlucky days—no work was done. UAY—unlucky, EB—finish of work (*i.e.* the sowing).

POP (July and August) the maize is now springing up. POP means leaves (suitable for thatching).

UO (August) the maize is now beginning to produce cobs. UO is a corruption of UOL—a general word meaning any round object, here the cobs swelling up on the stems.

ZIP (September) the corn is ripening. ZIP means seed that is swelling to ripeness.

ZOTZ (September-October) the stalks are bent down. ZOTZ means to lie on the ground (ripe maize has always a tendency to lie along the ground).

TZEC (October) the harvest begins. TZEH means to husk the seed.

XUL (November) the harvest finishes. XUL means end—the end of the harvest and the end of the agricultural year.

CAUAC, which forms the basal glyph of CHEN—YAX—ZAC—CEH, may connect with CAAC, to remove the leaves from the trees—a reference to the dry and lifeless period from January until the spring rains fall.

The birth of the New Year is thus made to correspond with the growth of the new maize crop, around which centred the whole life of the Maya.

Two courses are now open: either to accept Diego Landa's statement that the Mayans in some unexplained manner used leap years, or that the continual loss of days at the rate of twenty-four per century had caused in the course of some 1,540 years the loss of roughly a year, bringing the months back into correspondence with the seasons at about the time the Spaniards arrived.

These 1,540 years would carry us back to 8—12—0—0—0 II AHAU 3 PAX. In the chronicles we are told "POP was put in order" in the Katun 13 AHAU. It is a strange coincidence that the Katun 8—II—0—0—0 should be 13 AHAU.

The only known historical date preceding 8—II—0—0—0, that of the Tuxtla statuette (8—6—2—4—17), entirely lacks any month glyph, whereas the Leiden Plate of 8—14—0—0—0 has the month's sign Yaxkin clearly marked, although the coefficient is doubtful.

However, this is merely a tentative suggestion of a possible line of research; we are treading on very unstable ground, which, without present knowledge of cycle 8 times, is not likely to lead to anything more definite than possible theories, at present neither open to confirmation nor off-hand rejection. Any future correlation of European and Mayan dating will have to conform to the seasonal character of the months demonstrated above, and any—such as Mr. Spinden's—that make Cumhu and Pop midwinter must be rejected.

It is interesting to note that the famous date 6 CABAN 10 MOL, around which Spinden's argument revolves, is, according to the Landa dating, December 22nd—the winter solstice.

J. E. S. THOMPSON.

Pacific: Ethnology.

Ray.

Pearls as "Givers of Life." A Correction and Note. By S. H. Ray. 72

In MAN, 1924, 131, an error has been made inadvertently in the list of Melanesian ways of naming the pearl. On p. 183, line 15, the words "Florida and" should be deleted. The correct Florida name is given five lines above.

The Rev. Dr. Fox writes to me from San Cristoval that the word *dahi* besides meaning "pearl-shell" also means "to be successful." Also in the Sa'a (Malaita) dictionary of Dr. Ivens we may trace the succession of ideas. "Dahi, dehi. 1. The "golden-lipped pearl shell. 2. A crescentic breast ornament for men cut from "this shell. 3. In Ulawa, a phase of the moon. 4. To be favoured, to be lucky." Also, with the causative prefix: *Ha'*dahi. 1. To cause a person to be fortunate. "to thank, to make presents to. 2. When at sea to apostrophize the various "islands in sight." Dr. Ivens gives examples: *I sapena i Sa'a ni menu* (the configuration of Sa'a ni menu), addressed to Sa'a; *i sapena Ulawa e rara*, to Ulawa; *i sapena i Dara ahu'i niu*, to Ugi; *i sapena i Aru ni i'e*, to San Cristoval. In these

phrases we have: *menu*, bird: *ahu'i* to wrap up, protecting; *rara* to be hot: *niu* coconut; *i'e* fish. In his MS. vocabulary, Dr. Fox described the *dahi* ornament as representing the moon, worn only by the Araha clan and thought to bring good luck.

In his letter to me, Dr. Fox states: "Pearls were not prized. Very occasionally " I have been shown a small one, put aside as a curiosity, or given to children, but " they had no name for it, and just used *Kora* as they would for anything round " or new to them, and I should think this the case generally in the Solomon Islands."

SIDNEY H. RAY.

REVIEWS.

Polynesia: Sociology.

Williamson.

The Social and Political Systems of Central Polynesia. By Robert W. Williamson, M.Sc. In Three Volumes. Cambridge. At the University Press. 1924. Vol. I. xxix + 438 pp., Vol. II., 496 pp., Vol. III., 487 pp. Price 3*l.* 15*s.* **73**

In these volumes Mr. Williamson presents the results of his investigation of the social and political organisation of the people of the Central Pacific, that is, of Polynesia with the exclusion of New Zealand and the Hawaiian Islands. A great deal of more or less valuable material, recorded by various writers during the past century, has for a long time required correlation and classification. It has been drawn upon mostly to support various theories of origin and dispersal, but the cumulative evidence as to custom and beliefs has been very generally neglected. The presentation of an intelligible account of Polynesian Sociology called for an immense amount of research among the often scanty and contradictory records, for infinite patience in correlating and arranging the facts and an intensely critical judgment to decide how far the facts noted could be regarded as indications of the sociological development of the people. How thoroughly Mr. Williamson has performed his difficult task is seen in this work of nearly 1,500 pages, in which he discusses the statements of over two hundred writers.

In his first chapter the author reviews the various theories of Fornander, Percy Smith and Churchill as to the origin and migrations of the Polynesians. He agrees with the suggestion of a western origin, and apparently accepts Churchill's theory of a passage of the Polynesians through the Melanesian islands, though it is not clear whether he considers the present speakers of Polynesian in Melanesia to have come also from the west. An admirable summary is given of the theory of Rivers as to Melanesian and Polynesian social evolution, and its bearing on the possibility of more than one racial element in Polynesia.

Mr. Williamson finds grave elements of uncertainty, and a possible source of inaccuracy, in the calculation of ethnological time by generations. His objection is based on the uncertain duration of chieftainship and the influence of customs regulating the age of marriage. An open mind is kept as to Percy Smith's statement of three Polynesian migrations. For the first, that of the original Tongans and Samoans, Williamson suggests the name Pre-Rarotongan; the second—the people whose wanderings over eastern Polynesia are described in the Rarotongan log and who introduced the worship of Tangaroa—he calls the Tangaroans. A third migration was from Hawaii to Tahiti. The people of these three movements are the Polynesians of Mr. Williamson's book, but he does not deny the possibility of an earlier movement or movements of a people perhaps ethnically different from the so-called Polynesians, but who, nevertheless, were ancestors of the people now called Polynesians.

The first volume is devoted to an investigation of social areas and systems. The author's general plan is to classify and annotate first what has been written

about Samoa and Tonga, and thence proceed to the Society and Hervey Islands, the Marquesas, Paumotus and outlying islands. He is then led to an hypothesis as to their early history. He concludes that when the Tangaroans—the people whose movements to and in the Pacific are described in the Rarotongan logs—arrived in Samoa they found it occupied by a Polynesian population—the Pre-Tangaroans. The Tangaroans—so named from their god Tangaroa—established themselves first in Fiji and then passed to Tonga and the Samoan islands of Manu'a and Savai'i. The royal line of Upolu in the Samoan group was associated with the pre-Tangaroans and Mr. Williamson regards the genealogies as competitive, disclosing efforts of the Tangaroans of Manu'a to prove their superiority over the Pre-Tangaroans of Upolu, and attempts by the latter to resist this. The author suggests that the great gods of Polynesia, other than Tangaroa, were gods of the Pre-Tangaroans. Tangaroa's absence as a god in Fiji is ascribed to the squeezing out of the Polynesians by the Melanesians. These hypotheses are founded mainly on legendary evidence.

The history of Tonga, Tahiti and Ra'iataea, so far as it is known to Europeans, is stated by Mr. Williamson. Tangaroa was specially connected with Ra'iataea and from him the god Oro received dominion over air and earth, and became one of the great gods. Mr. Williamson suggests that Tane was the great god of the Teva people in Tahiti, and hence the reference of Ellis to the deities of highest order—Tangaroa, Oro and Tane.

The author finds in the Society Islands, as in Samoa, two competitive cults, that of Tangaroa and Oro his son, worshipped by the Tangaroans, and that of Tane, worshipped by the Pre-Tangaroans.

The second volume deals with the social and local grouping. The terms used by various writers were difficult to co-ordinate, but Mr. Williamson finds that in Samoa the domestic household was governed by one of its members. A collection of these households formed a village governed by the *fono* or council, with the head of one of the families as president. A collection of related villages formed a village district, which itself formed part of a district. For other islands information is defective or wanting, but the general conclusion is that grouping of a social character was similar to that of Samoa.

Mr. Williamson discusses the *marae* as a centre for the performance of social and religious rites. The conditions of entry were kinship.

Descent, exogamy and relationships are described in separate chapters. In Samoa the succession is found mainly patrilineal, but with ideas derived from matri-lineal descent. The special sanctity, powers, and privileges of a man's sister are based upon an ancient lingering belief that the family kinship and all that it involved was expressed through her.

The author finds no true totemism in Polynesia. But animals and objects were regarded as incarnations or associates of the god, and the spirits of the dead entered into animals. The objects marked on people's bodies, canoes, buildings and elsewhere are described as clan badges, and connected with the Social Grouping.

Organisations for warlike purposes are described shortly, but four very interesting chapters deal with the various orders of society—Chiefs, middle and lower classes, priests and sorcerers—and with the composition and powers of the *fono* or council.

The third volume discusses the powers and prerogatives of chieftainship and the administration of justice. The evidence seems to show that in Samoa and the Society islands, the head of a social group—a father of a family or a chief—was its natural priest. This was less certain in Tonga. The foundation of a chief's power was religious, based on a belief that it was derived from the gods. This is a stage further than the Melanesian belief in *mana* derived from ghosts. The sanctity of the chief was shown by his long ancestry and divine descent. Later, by the delegation of religious duties to others, the sacred and secular offices became separated. The

various powers of the chief, religious, administrative, parliamentary, consultative, military, diplomatic, judicial and personal, introduce a discussion on the relations between the various classes of society, in which Mr. Williamson suggests that the chief and their families were descendants of a conquering race, now become merged in the conquered. Names and titles, and their inheritance by testamentary disposition or election, are followed by an account of land tenure and control, the control of food supply, tribute and other matters relating to government.

It is impossible to enumerate here a tithe of the subjects dealt with in these volumes. Reference to the many items is, however, rendered easy for the reader by an excellent analytical index. Although Mr. Williamson has been to a great extent at the mercy of his authorities, his acute judgment has clearly distinguished the more from the less valuable, and produced a work which should be a standard of reference for Polynesian Sociology.

S. H. RAY.

Africa, West: Ethnography. Christy.

Big Game and Pygmies. By Cuthbert Christy, M.B., C.M. (Edin.). London : Macmillan & Co., Ltd. Pp. xi + 325. Price 21s. net. **74**

No traveller, not even Major Powell Cotton, has ever had better opportunities to study the Pygmies than those Dr. Christy enjoyed during his long stay amongst the Bambute in the Ituri forest; as a medical man and a trained scientific observer, he was specially qualified to gather a rich harvest of information concerning these elusive little folk. It is all the more disappointing to have the author's confession that, "during the time I was in the forest with the Pygmies, I was so occupied "in studying the secrets of forest-craft, and in my pursuit of the okapi, that I "neglected, now much to my regret, to note the habits of my little hosts." He had hoped for another opportunity, and it never came—it never does. A mighty hunter, he gained the confidence of the Pygmies and is able to record interesting facts concerning their armament, the use they make of their primitive weapons, and the more apparent details of their daily life. Bearing in mind his own confession, his statement that the Bambute have no conception of a deity in any shape or form has to be accepted with caution; it may be, and probably is, based on a lack of information from people who "are extremely shy and nervous when "holding intercourse with the white man or stranger." Dr. Christy has noted the very interesting fact that not only the Pygmies, but also the neighbouring tribes, use stripes and dabs of colour on the body and face to make themselves less conspicuous in the forest by breaking up the outlines of the figure. People of the grass-land in the Congo protect themselves, though they may not do so consciously, with red paint against the actinic rays; the origin of painting seems to be in both cases purely utilitarian.

Having made a special study of the relationship between animals of the same species dwelling in and outside the forest belt, Dr. Christy comes to the conclusion that the tribes inhabiting the margins of the Ituri forest, though several inches taller than the Pygmies and darker in colour, are practically the same people as the Bambute, altered by changed conditions of life in consequence of their being gradually pushed out of the diminishing forest. Other travellers have observed that Pygmies whom the force of circumstances has driven into the savannah undergo changes of this kind; but in the Ituri region the presence of the marginal forest tribes is the cause of the diminution of the forest—the only possible cause and not the result. They are the *avant-garde* of the "eaters of the forest," the Bantu, who are cutting their way into it by clearing yearly fresh ground for their plantations. There is plenty of room in the remaining huge forest belt for the nomadic small hordes of Bambute; and what we know of them, as well as what Dr. Christy himself says of them, makes it incredible that they should voluntarily

take to a semi-agricultural life. Their apparent structural resemblance to their bigger neighbours is not limited to the "marginal forest tribes"; the full-sized Bantu of the grass-land on the northern bend of the Congo are of the same square, short-legged, sturdy type as the Bambute, while further south, as in the Sankuru, both Pygmies and Bantu are of a slighter, slender build, with longer legs in proportion to the body. The possibility of the Bantu marrying or raping systematically Pygmy women is precluded by the superstitious awe for, and the salutary fear of, the vindictive little people; consequently, we have still to search for a satisfactory explanation of this division of both Pygmies and Bantu into two distinct structural groups.

The book is lavishly illustrated with excellent photographs, mostly by the author, and contains a great wealth of matter which will be of interest to the naturalist and the sportsman.

E. TORDAY.

England: Place Names.

Mawer and Stenton.

English Place-name Society, Volume I. Introduction to the Survey of English Place-names. Part 1. Edited by Allen Mawer and F. M. Stenton. **75**
Part 2. The Chief Elements used in English Place-names. Edited by Allen Mawer. Cambridge University Press. 1924. Pp. vi + 67. Price 21s. net.

The English Place-name Society has undertaken a great work, and has been fortunate in its supporters, who can bring a varied experience to the solution of its problems. The first part of this volume lays down the principles according to which it proposes to work, and these are described by various members of the Society's Council. Professor Sedgefield describes "Methods of Place-name Study"; Professor Ekwall contributes two chapters on "The Celtic Element" and "The Scandinavian Element"; Professor Stenton two more on "The English Element" and "Personal Names in Place-names," while Mr. Crawford has a chapter on "Place-names and Archaeology." There are further contributions by Professor Tait, the President, Mr. Zachrisson, Professor Wyld, and Miss Serjeantson. The second part is by Professor Mawer, the Secretary and Director.

The earliest attempts at explaining the meaning of place-names were those folk-etymologies which were as amusing as they were unscientific; these were followed by the efforts of compilers of parish and county histories, who were anxious to extract from the names some evidence of a romantic past, something relating to a Christian missionary, the hero of a saga, or a pagan shrine. Then came the philologists, paying strict attention to the laws of phonology and Old English grammar. These worked out successfully the origin and meaning of many of the names; but as they refused the assistance of those acquainted with the topography and the archaeology of the districts in question, and were obsessed by an obstinate Teutonism and a passion for personal names, their works were disfigured with many etymologies which seemed ridiculous to those familiar with the locality.

The present group of workers is anxious to avoid these pitfalls, and has taken steps to consult experts in those studies which are likely to help; it is anxious, too, to avoid an undue preference for Saxon origins. Crawford, it is true (p. 153), makes much of the hiatus between British and Saxon times: but Sedgefield (p. 22) notes that "British names of towns, villages, rivers, etc., are found all over England"; Ekwall says (p. 31): "The numerous British place-names found in England prove that the British population cannot have been exterminated or swept away, even in the parts first occupied by the Anglo-Saxons"; while Stenton remarks (p. 181) that "the existence of a Celtic element in the body of Early English personal names is certain."

Crawford's treatment of the archaeological evidence is suggestive, especially in the case of *Stodfold*. That topographical evidence is necessary is recognised by Stenton, who has, however, used it imperfectly in the case of *Geatingadenu* or

Yattendon. Had he studied the geological conditions more thoroughly, he would have noted that, though most of the parish lies on the Reading beds and was therefore wooded, there is a considerable area in which the chalk reaches the surface, and that there is evidence that the village originally was situated within this area. The whole work, however, gives evidence of a sane and unprejudiced approach to the subject, and promises well for the quality of future volumes. H. J. E. P.

CORRESPONDENCE.

Sociology.

Long.

*To the Editor of MAN.***Problems of Social Organisation.**

SIR,—In MAN, 1925, 33, when comparing the marriages permissible under a dual organisation according as descent is matrilineal or patrilineal, Mrs. Seligmann says: "Of these the cross-cousin marriage, the marriage with the father's "brother's wife, the mother's brother's daughter and the brother's daughter's "daughter, would be allowed whether descent were in the male or female line." This overlooks the fact that the mother's brother's daughter is a cross cousin, and the more important fact that marriage with the father's brother's wife is prohibited in a dual system with matrilineal descent, as already noticed by Mr. W. E. Armstrong (MAN, 1925, 25). But this last marriage is permissible if descent is patrilineal. Again, when mentioning marriages permissible in a dual organisation with patrilineal descent, she says "These are with the mother's sister, the mother's "sister's daughter, and the sister's daughter." It is not the case that marriage with the mother's sister's daughter would be permissible with male descent. In such a case, my mother and her sister are both in the moiety into which I can marry, but as they must marry men of my moiety, then, under male descent their daughters are of my moiety and forbidden to me.

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The general proposition may be laid down that all marriages in the same generation as Ego or with members of generations an even number above or below (e.g., grandparents or grandchildren) follow the same rules in a dual system whether descent is matrilineal or patrilineal, but with generations removed an odd number of times (e.g., parents, children or great grandparents), the permissible marriages under one rule of descent are the reverse of those under the other.

Yours, etc., RICHARD C. E. LONG.

Technology.

Thomas.

Netting without a Knot.*To the Editor of MAN.*

SIR,—With reference to my note published in MAN, 1924, 113, under the above heading, the following two additional examples should be cited:—

77

1. Aboriginal Tasmanian basketwork. Shown in Fig. 1, which is taken from Ling Roth's "The Aborigines of Tasmania," p. 145. There it is stated that there are examples of this work both with and without the horizontal strands.

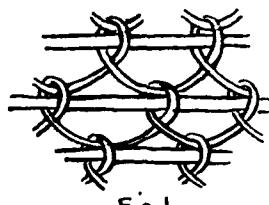


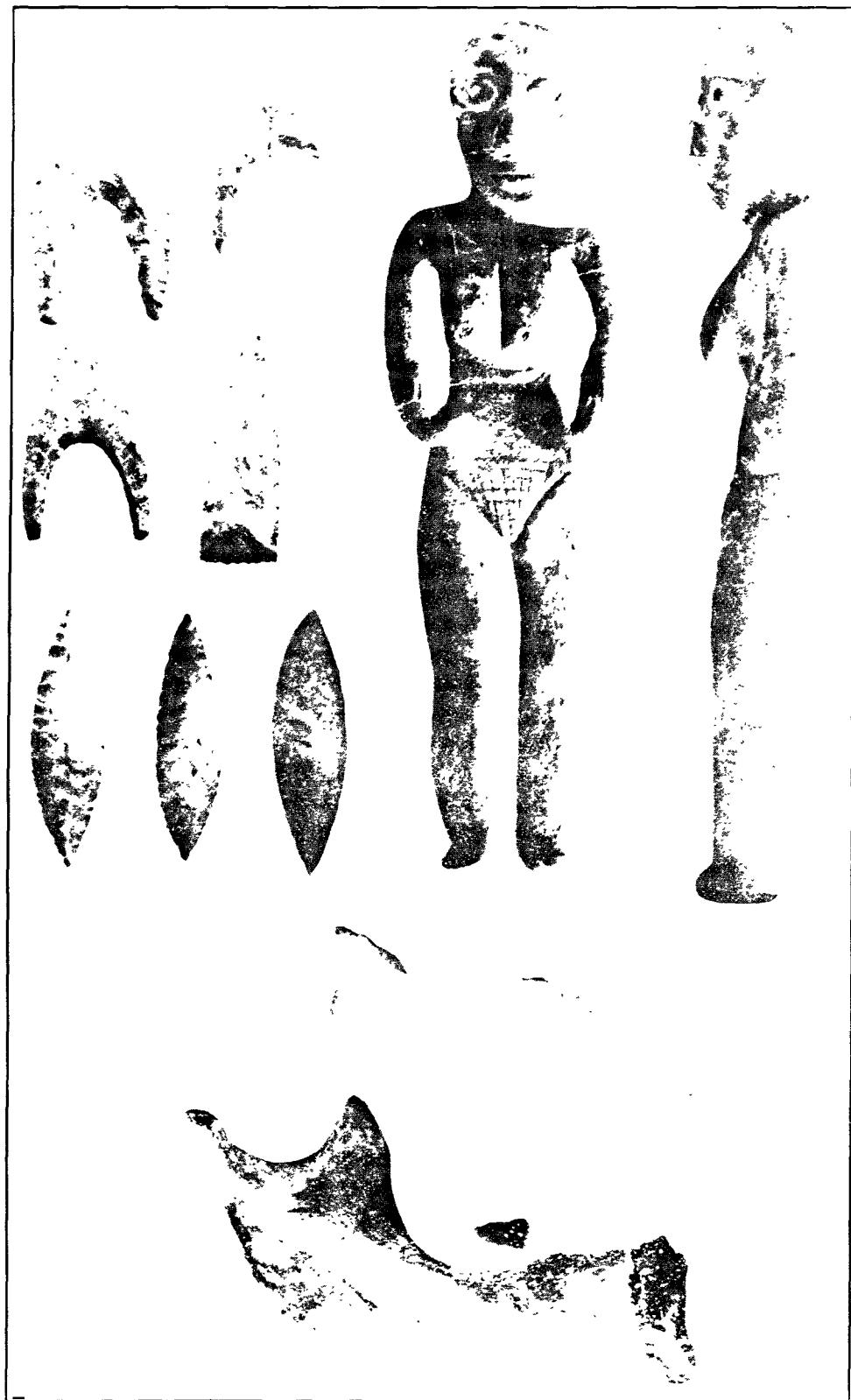
Fig. 1.



Fig. 2

"endless," the looping done spirally upon triple threads wound upon a ring of upright sticks. This fact is stated, I think, in Burrow's "Land of the Pygmies."

Yours, etc., ERNEST S. THOMAS.



EARLY MAN IN EGYPT.

ORIGINAL ARTICLES.

With Plate I-J.

Egypt: Archæology.

Petrie.

Early Man in Egypt. By Sir Flinders Petrie, D.C.L., LL.D., F.R.S.,
F.B.A. With Plate I-J. **78**

During the last three winters the history of man in Egypt has been much extended by the work of the British School of Archaeology in that country. An earlier civilisation than any yet known has come to light, which made the finest pottery, glazing, and ivory carving, with flints of Solutrean type. In the district of Badari, 30 miles south of Asyut, on the eastern side, a settlement was found, having 2 feet of late prehistoric pottery at the top, 2 feet of earlier prehistoric below, and 2 or 3 feet of the pottery and flint work of the earlier culture at the bottom, with walls of huts still in place. A layer of rubble breccia, fallen from cliffs above, was intercalated in the lower stratum, showing that it was extended over a long period.

A few miles to the north, cemeteries were excavated, in which was the same kind of pottery, some of it the thinnest and hardest known of any age, and with a polish never surpassed. This was entirely handmade, and worked down with combing diagonally each way so that no irregularity in thickness can be found. Hæmatite polish was used, much of it burnt black in ashes. Two statuettes were found; one, of ivory, was of a heavy type, suggesting an Asiatic source (*see plate I-J*); the other, with a slender waist, is of red pottery, of a form like the Cretan. Malachite eye-paint was used, ground on slate palettes. Green glaze is frequent on small beads of schist. Ivory was much used for combs, spoons, and other purposes. The flint work reached a very high stage of delicacy in the small arrow points, both the swallow-tail and tanged forms were used. The forms are like those of Solutrean in these and also in skew-points, adzes, vesica forms, prismoids; and in the Fayum also the laurel leaf.

Flint work of these types was already known largely from the Fayum, and across the desert up to Palestine. Last winter in the Fayum several settlements were examined. Some are hundreds of feet across, and 5 or 6 feet deep. These sites, and the flint work with pottery, extend down to a level which was covered by High Nile since 15,000 B.C. and even by Low Nile since 12,000 or 13,000 B.C. The regular rise of deposit in the Nile valley serves as a time scale, and the most rapid rate known is here adopted, so the age cannot be shortened. After the sites were covered by water the Ptolemies stopped the inflow to the Fayum basin, and dried it up in order to acquire cultivable land. In this way the desert surfaces on the north side have been exposed, which were formerly flooded from perhaps 50,000 B.C. It is thus a unique ground for searching dateable deposits; unhappily it has been raided mercilessly by natives, so that little can be found except in mounds. All unlevelled collecting should be entirely stopped. The main difficulty of continuous work has been the water supply, which has to be brought from 15 miles away. The settlements have been the ground of Miss Caton-Thompson, while the cemeteries have been worked by Mr. and Mrs. Brunton.

The historic position seems to be that the Solutrean folk of Europe came westward from some Asiatic centre, while a branch of the same culture was taken southward to Egypt. The similarity of period agrees with the uniformity of types of flint work. The centre of this culture may have been in the Caucasus or by the Caspian: and the resemblance of Colchians to Egyptians, stated by Herodotus, may be remembered. The Egyptian group passing through a good climate and easy conditions kept up the Asiatic culture, and brought some beginning of polished stonework, which died out in Egypt. The European group hunted and fought

their way west along the glacial fringe, and could not carry pottery, or care about glazing, but yet kept their artistic work in flint and in rock carving, as at Bourdeilles (*Comptes Rendus*, 1925, p. 50).

Of a vastly earlier age are the portions of human skulls, jaws and other bones, found with an immense quantity of hippopotamus bones, from the gravels of the ancient High Nile at Qau, a few miles south of Badari. These gravels, of felspar pebbles and clean quartz sand, extend to 100 feet over the plain, with at least 30 feet more of calcified marl and breccia over that. Thus the fossilisation of the bones, the rolling down in the stream beds, the deposit of thick gravels, and the recession of the Nile from the high level all took place before the Chellean age; for the flints of that time and of the Acheulian and Mousterian, all lie over the High Nile gravels, and at all levels down to the present Nile, unworn and bare on the surface. The back of a skull here is of the modern type in its size and thinness; the female jaw (upper one on the plate) is delicate, and hardly different from that placed below it, which is of the best age of the Pyramid builders. Thus the highest type of man is apparently dated to pre-Chellean ages. The search for the original swamp deposits by the eastern igneous mountains, whence the gravels came, and where the hippopotamus abounded, is now one of the most promising openings for a geologist.

FLINDERS PETRIE.

America, North: Technology.

Hemming.

A Notable Wampum Belt. By Arthur G. Hemming.

79

In the accompanying illustration is figured a fine Wampum belt which has recently come into my possession.

The shell belt, apart from the ends of hide, measures $24\frac{3}{4}$ inches long and $4\frac{1}{4}$ inches broad. It consists of 2,696 beads in 16 rows on a woof of thread with a warp of hide, of which 1,583 are white beads and 1,113 are purple. At the end of one of the leathern thongs is a small pouch in the form, apparently, of a tortoise, in which presumably a charm was carried.

There is, of course, a considerable literature on the subject of Wampum, some of the papers containing interesting illustrations and it may be convenient if I give

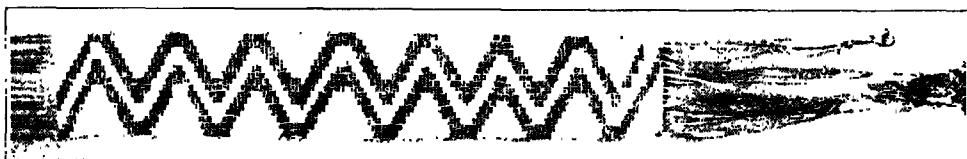


FIG. 1.—WAMPUM BELT OF THE IROQUOIS CONFEDERACY.

the following references, which, without being an exhaustive list, furnish a good deal of information:—

“Museum Tradescantium or a Collection of Rarities preserved at South Lambeth, near London, by John Tradescant.” London, MDCLVI.

“Catalogue and Description of the Natural and Artificial Rarities belonging to the Royal Society and preserved at Gresham College, London. 1681.”

“Art in Shell,” by W. H. Holmes 2nd Ann. Rep. Bureau of Ethnology. Washington, 1883.

“Four Huron Wampum Records,” by Horatio Hale. *J. Anth. Inst.*, Vol. XXXI, 1897, p. 244.

“The Origin of Wampum,” by D. I. Bushnell, Junr. *J. Anth. Inst.*, Vol. XXXVI, 1906, p. 172.

.. “Flint Chips,” by Edward T. Stevens, Hon. Curator, Blackmore Museum. 1870, p. 454.

The chevron pattern in the example figured indicates, apparently beyond doubt, that the belt belonged to the Iroquois Confederacy. Mr. Hale, who first brought forward this point in 1884, states that the origin and meaning of the oblique band were well understood among the Indian tribes, and in the paper referred to above, "Four Huron Wampum Records," he produces valuable corroboration of his statement by the important authority of two chiefs well versed in Iroquois tradition, G. H. M. Johnson (Onwanonsyshon), the Government interpreter, and John Buck (Skanawati), the official keeper of the wampum, whose father, grandfather, and greatgrandfather had held the same office.

Among the Iroquois wampum beads were used chiefly for religious purposes and by way of ratification of laws and treaties. ARTHUR G. HEMMING.

Papua: Sociology.

Lyons.

The Significance of the Parental State amongst Muruans. By A. P. Lyons.

80

Muruua or Woodlark Island, situated to the east of the Trobriand Islands, is part of the territory of Papua. Its people, who are included in the Northern Massim group, observe matrilineal descent and inheritance. They are organised into eight exogamous clans or Kums. Members of each Kum inherit a *man* (bird) totem and a linked (fish) totem as well, and two of those clans also inherit a snake totem.

The Muruan has no special word to denote totem, but as the principal totem of each Kum is a bird, the word *man*, by which birds generally are designated, is employed. Each species of birds has a distinctive name.

If asked "What is your Kum?" a clansman will understand you to be inquiring the name of his totem bird. Similarly, if asked what is his fish, he will tell you the name of his linked totem. He will not avoid either killing or eating his own totems, but he will neither kill nor eat those of his father, and he simulates grief if he sees anyone else doing so, giving as a reason, "that that is his father."

Youthful marriages are customary amongst the natives of Murua. Indeed, it is their practice to celebrate a marriage soon after the parties thereto have reached the age of puberty.

The birth of the first child of a marriage marks the commencement of a ceremony called Vaiaura, which, besides being a purificatory function, affects the relationship between, and the status of, the husband and wife, and subjects the child to the same strict observance of a taboo that is then placed upon its parents.

During parturition, and for a period of from one to two months thereafter-wards, the woman and her first-born child are confined within a temporary room that is constructed for the purpose at the women's end of her mother's house. A fire is kept constantly burning within the room, in the smoke from which the woman sits with her infant. It is thought that smoke will prevent some lurking evil spirit from harming the mother and her child, who, at this particular period, are most susceptible to injury from unseen forces. As a further safeguard, the mother frequently anoints both her own and her child's body with cocoanut oil. Food is provided and prepared for her by her mother. Pig, fish and the meat of cocoanut are forbidden to wife and husband while the former is living in seclusion with their child.

Meanwhile, the husband's father makes preparations for the feast that will terminate the Vaiaura ceremony. When they are completed, and the relatives of the young parents are assembled at the place appointed for the feast, the mother and her child are conducted thither by her mother and other female relations.

The father of the child accompanies his father to the feasting place.

Before leaving her place of seclusion, the young mother's head and that of her child are anointed with cocoanut oil by her mother, to prevent any harm befalling them on the way to the feast. Similarly the head of the father of the child is anointed by his father. At the meeting place, the young parents are brought together, and then the husband's father, in solemn form, imposes on them, as well as on their child, a strict and lifelong taboo of certain fish which he names. Afterwards he anoints their heads with cocoanut oil, when the ceremony ends, and feasting and dancing are indulged in.

Vaiaura is held only after the birth of a first child begotten of a lawful marriage. It is not held after the birth of a subsequent child, neither is it held upon the occasion of the birth of a child to a marriage one party to which was a widow or widower who had previously begotten a child and the other was not.

It would be held, however, if the widow or widower had not begotten a child by the first marriage.

The first-born child, then, observes this fish taboo all through life. If a female child, she would, upon becoming a mother through marriage with a single man or childless widower, have to observe an additional taboo of the fish named by her father-in-law on the occasion of and from the Vaiaura following the birth of her first child. That would not be so in the case of a man who was himself a first-born child, for he has had to observe his father's taboo from birth.

In the case of a second born or subsequent child, it receives the fish taboo at the Vaiaura held after the birth of its first child.

From what transpires after the birth of a first child, and at the Vaiaura ceremony, it appears as if the Muruan regards the incidence of child-birth as being something in the nature of a welding of personalities,—in other words, a trinity of father, mother and child.

A. P. LYONS.

India: Technology.

Cammiade.

A Primitive Vehicle on Runners. *By L. F. Cammiade.*

The vehicle (Fig. 1) consists of a basket mounted on two converging runners and a yoke at the apex of the runners. It was in use in the roadless area

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near the jungle village of Jadangi, Yellavaram Taluk, Godavari district.

When in use only the lower ends of the runners touch the ground.

The vehicle is usually drawn by a pair of bullocks: the central bullock in the illustration is being trained to the yoke.



FIG. 1.—PRIMITIVE VEHICLE, GODAVARI DISTRICT, INDIA.

The advantage of the vehicle is that it can be used over rough, stony ground that would smash the wheels of an ordinary country cart, and it can slide down and up muddy water-courses and gullies where an ordinary cart would stick.

Is this vehicle a survival of an ancient pre-wheel type, or was it a comparatively recent invention to meet local needs?

Possibly it is a survival:—

1. Though wheeled vehicles have been known in India for about three thousand years and more, there were no roads practicable for wheeled vehicles in

the region round Jadangi until about fifty years ago, and to this day very few roads exist.

2. A similar vehicle is in use at the village of Pôlavaram, on the banks of the Godavari, about fifty miles S.W. of Jadangi. This vehicle is used for fetching drinking water across the half mile of sand and silt that lies between the village and the river during the greater part of the year. A large brass pot takes the place of the basket. The runner carriage has survived at Pôlavaram, which is a civilised place, because it is better adapted for travelling over loose sand and soft silt than country carts.

L. F. CAMMIADE.

China: Mythology.

Waley.

A Note on Two New Studies of Chinese Mythology. By Arthur Waley. **82**

Archæology* has at last succeeded in establishing a link between the material culture of pre-historic China and that of the West. Meanwhile, two attempts have recently been made to trace parallels between Western and Chinese mythology. Speculation of this kind has been somewhat discredited by the fanciful theories of Terrien de Lacouperie and his followers; it is as well, therefore, to say that the researches which I am about to notice are conducted in a very different spirit.

(1) In *Asia Major* (Vol. I, 1924, pp. 550-586), Dr. Otto Mänen-Helfen discusses the later books of the Chinese *Shan-hai-ching* ("Canon of Hills and Seas"), and in particular the relation between the Abode of the Blessed, as there described, and the *Uttarakuru* of the *Rāmāyana* and *Māhābhārata*. It is noteworthy that Dr. Mänen-Helfen nowhere discusses the date of the Indian sources, but seems tacitly to assume that they are older than the Chinese works from which he quotes. This is all the more remarkable, because, whatever age one may attribute to the content of the *Māhābhārata*, its actual composition was (according to average current opinion) considerably later than that of *Lieh Tzü*, *Huai-nan Tzü*, the *Shih-chi* and other Chinese books which describe the Abode of Bliss. That the conceptions of Paradise with which he is dealing are not peculiar to India and China is recognised by Dr. Helfen. He concedes that Persia may have contributed something, and even cites a Teutonic parallel. But the criticism which I wish to make (not with regard to his publication of Chinese-Indian parallels, which is most valuable and interesting, but with regard to the conclusions which he draws from them) is this. To trace a parallel between conceptions in two countries and then to conclude, without attempting to fix the relative antiquity of these conceptions, that country B is indebted to country A, seems to me to be wholly unscientific. Dr. Helfen does indeed bring in a third country (Persia), but he decides, owing to the existence of two features† which are common to the Indian and Chinese traditions, but do not occur in Persia, that India and not Persia was the source of the Chinese legend.

We are dealing here, however, with conceptions which are current all over the globe. The only form of enquiry which could be profitable would be a study of these conceptions *wherever* and *whenever* they occur. We should rapidly discover that in some parts of the world they were demonstrably far older than in India, and we might in the end, by an historic enquiry of this kind, succeed in showing where the ideas in question originated and by what routes they spread. Incidentally we might easily discover that features of the legend preserved in India and China, but not in Persia, were equally preserved in, say, Mexico or Africa, and

* e.g. "An Early Chinese Culture," J. G. Andersson, *Bulletin of the Geological Survey of China*, No. 5, 1923.

† It is only fair to say that his argument is not here complete, but will be further substantiated in his next article.

the whole theory of pre-historic Indian influence on China might have to be abandoned.

(2) The other work which I wish to notice is Professor Alfred Forke's *The World Conception of the Chinese*. Probsthain, 1925. The author follows each section of his book with a series of parallels from other countries. "Most Australian Natives," "the Dacotahs," "Pythagoras," "the Chaldeans," "the Dacotahs," "the Wotjoballuk" trail, in a rather bewildering succession, after each Chinese quotation. Not that Professor Forke is a "diffusionist." On the contrary he attributes (p. 244) the parallels which he adduces to the working of "human nature " which is more or less the same everywhere."

It is perhaps rather late in the day to refute this odd view. But among writers upon Far Eastern subjects, I notice that it is still very widely held, and one consideration with regard to it may be worth putting forward. What reason is there to suppose that the religious ideas of the pre-historic period (e.g., the conception of an Abode of the Blessed) did not spread in exactly the same way as the religious ideas of a slightly later time? If Professor Forbes were to find the same story (say of a prince riding out of his palace and meeting a sick man) in Ceylon and among the Kalmucks of Tartary, he would not attribute this to the operation of "human nature which is more or less the same everywhere"; he would recollect that, starting from a central point in northern India, this legend of Shakyamuni spread almost to the Arctic zone on the one hand and the Equator on the other. Surely if early ideas are to be investigated at all, the object of such an enquiry must be to turn pre-history into history; to give an account of primitive conceptions which shall, so far as is possible, trace their origin and diffusion, and bear at any rate some resemblance to the account which religious historians are able to give of the rise and spread of Buddhism or Christianity?

ARTHUR WALEY.

Britain: Archæology.

Cunnington.

A "Thames Pick" of Iron Age Date. By Mrs. M. E. Cunningham.

83

This flint axe (Fig. 1) was found on a ledge or shelf in the chalk side of a pit-dwelling in Casterley Camp, Wilts. The pit was an unusually large one, measuring 17 feet by 15 feet, and 6 feet deep. The skeletons of three adults and

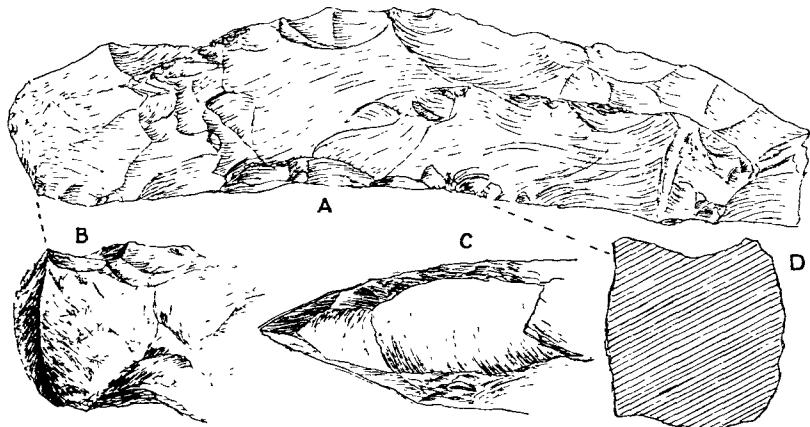


FIG. 1. -FLINT AXE FROM CASTERLEY CAMP, WILTS (½ SIZE). A, SIDE VIEW. B, NOSE. C, VIEW OF NOSE FROM ABOVE. D, SECTION.

one child were found lying on the chalk floor. and, under one of these, the remains of an iron brooch with spiral spring of La Tène type, and under the child's skeleton a second fragment of iron, perhaps the bow of another brooch. The

report of the discovery states—"The brooch was a fortunate find as it shows that "these pit people belonged to the Early Iron Age, for otherwise the poor hand-made quality of the pottery, and the finding in this pit of a flint axe and flint flakes, might have given rise to the supposition that the pits were of a much earlier date." (*Wilts. Arch. Mag.*, Vol. 38, p. 77.) The axe, 8½ inches in length, is a fine example of the type known as a "Thames pick." The flint is of a pale bluish grey, the unground sharp nose shows no sign of use, and the whole surface is as sharp and rough to the touch as a newly knapped flint, as no doubt it was, when buried in the pit. Implements of "Thames pick" type, it seems, are usually regarded as of late Palæolithic or early Neolithic date, but the circumstances in which the Casterley example was found shows that they continued to be made in Britain well into the Iron Age.

M. E. CUNNINGTON.

Algeria: Technology.

Hilton-Simpson.

"A Fish-Trap from the Aures Massif." By Capt. M. W. Hilton-Simpson.

84

In March 1924, in company with Mr. J. A. Haeseler, I found myself in the oasis of Mechounech, which lies at the southern foot of the central part of the Aures *massif* in S.E. Algeria.

The inhabitants of this oasis are mostly of Shawiya (Berber) stock.

I there encountered a native, whom I had met in years gone by, carrying an object made of halfa-grass which he informed me was a fish-trap (Fig. 1).

It was made of a roughly plaited halfa-grass ribbon, sewn together spirally, so as to form a narrow tube, closed at its narrowest end, with a very wide, open bell mouth. The technique of its manufacture resembled that of ordinary Shawiya basket work, save that its ribbon was but loosely sewn together so as to allow the stream to flow through it rapidly.

The trap is used as follows. The narrow closed end is placed in the brook between two boulders (through which the stream runs fast from a pool), the wide open end being held in place beneath the surface by these boulders, facing upstream. A number of persons then "bombard" the pool above the trap with stones in order to drive the fish to seek refuge down-stream. Swimming out of the pool through their usual channel between the boulders, the fish enter the wide end of the trap and are swept by the current and their own impetus into its narrow closed tube, where they are unable to turn round and so to make their escape.

Haeseler recorded the use of the trap by means of his kinema camera.



FIG. 1.—FISH-TRAP, OASIS OF MECHOUNECH.

I had never previously seen a fish-trap in the Aures (where fish are small and scarce), so I quickly collected the specimen before me for the Pitt-Rivers Museum, Oxford. Upon enquiry, I learned that it came from the purely Shawiya village of Menaa. Had this occurred in 1913 I should probably (and perhaps excusably) have imagined that I had gleaned an item of information with regard to Shawiya methods of fishing. But I have known Menaa and most of its inhabitants well for the last dozen years and I had heard no whisper of a fish-trap in the locality, though I have accompanied its natives on fishing excursions. A very few enquiries served to show the origin of the trap.

None of the Shawiya I talked to had ever seen such a trap until three years ago. From 1917 to 1922 a certain Shawiya brigand, named ben Zelmat, caused something like a reign of terror throughout the Aures. To attempt his capture bodies of troops were employed by the French. The soldiers stationed at Menaa for this purpose were drawn from a Senegalese battalion, quartered in and around Biskra. One of these men, anxious to vary his ration diet, had made a fish-trap, of a West African type, from the material locally used in basketry, halfa-grass.

The trap, thus introduced, owing to the "exigencies of the Service" in times of turmoil, may possibly become popular in the Aures; although I do not think fish are sufficiently large or numerous to cause its use to become general.

At any rate its discovery at Mechounech has provided me with a lesson to the effect that investigation is necessary before labelling some specimens "Shawiya."

M. W. HILTON-SIMPSON.

British Columbia: Archæology.

Smith.

A Prehistoric Petroglyph on Noeick River, British Columbia. By **85**
Harlan I. Smith, *The National Museum of Canada, Ottawa.*

There are known to be seven groups of prehistoric petroglyphs in the Bella Coola Indian area of British Columbia and its immediate neighbourhood. Only two or three times as many petroglyphs are found on the whole Pacific coast of Canada, about half as many in the Interior of British Columbia, in each of the Prairie Provinces and in Ontario. There is one noted petroglyph in Nova Scotia. Judging from the data at hand there are in the Bella Coola area, perhaps, one-fifth of the petroglyphs of the whole of Canada. This area, not over 52 miles in greatest diameter, is approximately half way between Vancouver and Prince Rupert or at a central point on the Pacific coast of Canada.

The seven locations from north to south are as follows:—(1) the north side of the cañon of Dean river, about 2 miles above its mouth; (2) the shore of Dean channel, immediately north of the mouth of Swallow creek or approximately 11 miles south of the mouth of Dean river; (3) the beach at the eastern side of the entrance of Elcho harbour; (4) west of the falls about 2 miles south of Bella Coola; (5) west of the cañon about 1 mile south of Bella Coola river at a point approximately 3 miles above its mouth; (6) the north side of Bella Coola river opposite the mouth of Noosatsum river; (7) the south side of the mouth of Noeick river, South Bentinck arm. These groups are all located in the Bella Coola Indian area, except the one on Elcho harbour, which is in the Bella Bella area.

The present note on the prehistoric petroglyph on the south side of Noeick river being complete as far as our knowledge goes may be presented in advance of the report on all the petroglyphs. Noeick (Nooek, Noch, Nuik) river empties into the eastern side of South Bentinck arm about 5 miles from its head at a place 1 mile below or north of the mouth of Taleomay river and the Bella Coola Indian village of Talio. (Taleomay, Taleomey, Taleoniy, Tikunclose, meaning "I see your face.")

The petroglyph is on the rocky highland above the tide flat of the delta of the two rivers. To be exact, it is on a slope perhaps 45 degrees towards the north, that is, towards the Noeick on Lot 1139, Range 3, Coast district, 30 to 40 feet above the water. It is about 100 feet, estimating 5 feet to a double pace, east of the western edge of the foot of the rocky bluffs at this point, which in turn is about 35 feet east of the eastern line fence of a small Bella Coola Indian garden and plum orchard. This plot, partly on the lowland and partly on outcrops of rock, west of the main bluff line, is a small rectangular Indian reservation, but the fence is not exactly on the lines, as it encloses an irregular rather than a rectangular area. The petroglyph is about 120 feet south of a vertical cliff along the river. A little to the east there is about 20 feet of bottom land between the rocks and the water when the river is low.

One trail up the south side of the river goes over the rocks between the petroglyph and the river close to the latter. Another passes over the top or southern part of the rock exposure on which the petroglyph appears.

The slope of the rock for a space of about 20 feet long by about 20 feet wide is said to have been used for a slide by men, women and children, who slid for fun,

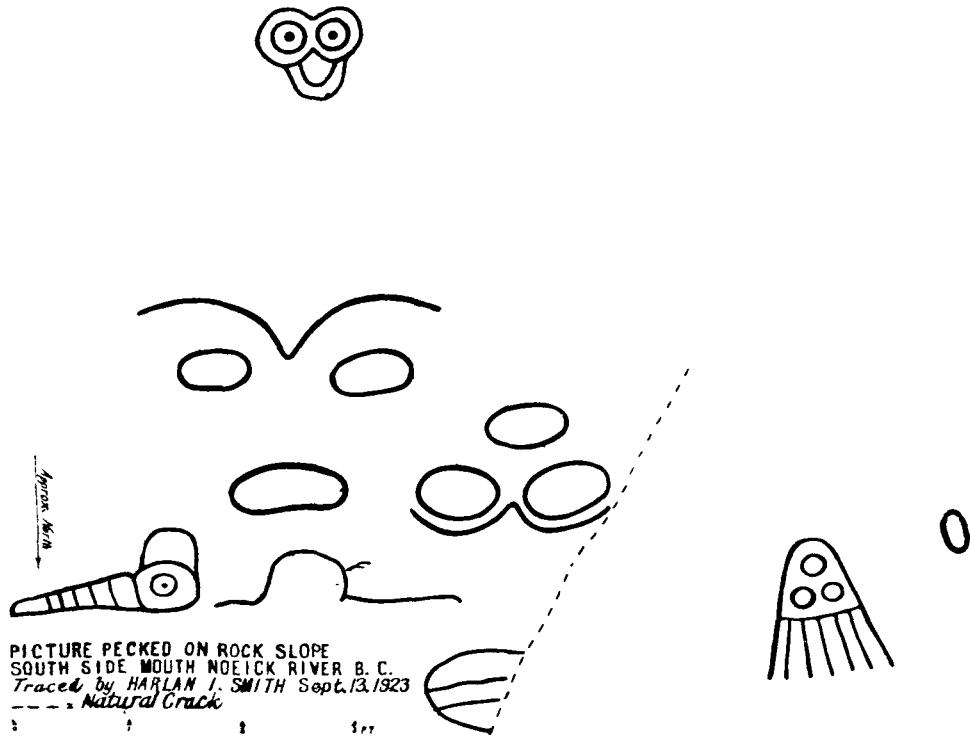


FIG. 1.—TRACING OF PREHISTORIC PETROGLYPH, NOEICK RIVER, BRITISH COLUMBIA.

seated on boughs of hemlock or other trees or on skins. The late Captain Schooner, an old Bella Coola Indian, said they slid in the summer, using boughs of any kind.

The petroglyph was pecked in the granite (Cat. No. XII-B-1584, The National Museum of Canada). The lines average possibly half an inch wide by a quarter of an inch deep. They are faint in places and difficult to see, except in certain lights. In fact, some cannot even be distinguished from the surrounding rock-surface by feeling. Their character and that of the field make their reproduction by rubbings difficult or impracticable. They are also somewhat obscured by moss, lichens and over-burden. All that could be found of the petroglyph was traced 13th September,

1923. Fig. 1 (58362) illustrates a photograph of this tracing. (Films 58761-58764 and a moving picture film were also taken of it.) The petroglyph shows at the top a human or other animal face or at least two eye-like figures. About 2 feet below is a figure which appears to represent eyebrows, two eyes and a mouth. At the right of the lower part of this is an inverted figure similar, though smaller. Below at the left is a head with an eye and an ear with a spiral, or a nose or beak, with cross lines, extending from it towards the left. This with the line to the right and the figure below it may represent a bird form, the lower figure being the tail. To the right, beyond a natural crack in the rock, is a figure resembling those so commonly found among the pictographs of the interior of British Columbia and which the Indians there interpret as bear tracks. But here the toes are indicated by seven lines instead of five. The ball of the foot bears three circles, which may represent an inverted face on the bear paw. However, the whole figure may represent a face, the seven lines being interpreted as hair. Further to the right is a small oval which seems to stand alone.

What this petroglyph represents was either unknown in full or unexplained to me. Captain Schooner said that some Bella Coola Indians of the Talio group might be able to explain the meaning of it. Such carvings among the Bella Coola are sometimes capable of explanation only by the family or group concerned, and unintelligible to the others because of the lack of distinguishing features. Captain Schooner* stated that one of the figures represented the haohao and that he had a mask or headdress depicting this being. Another, he said, represented the sniniq. The following information about the haohao, which is a mythical bird of the Bella Coola, has been given me by Mr. T. F. McIlwraith, who obtained it in the course of ethnological researches among the Bella Coola Indians. It has a spread of wing of about 6 feet, and a long beak furnished with teeth. Instead of feathers the bird is covered with bone-like scales. This bird was formerly fairly common. One was heard near Bella Coola about 1916. Professor Franz Boas gives an account of the haohao on page 100 of his report on the "Mythology of the Bella Coola Indians," published as part 2, Vol. I, of the Publications of the Jesup North Pacific Expedition, in Vol. II of the Memoirs of the American Museum of Natural History.

The sniniq has also been explained by Mr. McIlwraith as a mythical animal of the Bella Coola. It is about the size of a large grizzly bear, with short front legs and long hind ones. It has longish blue-gray hair and the sharp front legs terminate in the talons of an eagle. A sniniq usually carries a basket on its back in which to thrust its quarry; the basket provided with spikes pointing downwards on the inside, prevents a captive from escaping. When the animal reverses its eyes in their sockets there comes forth a piercing beam of light which strikes anyone senseless in its course. These animals were formerly fairly plentiful, but have recently become rare. No living Bella Coola has seen one, although several have been heard. In February, 1924, a number of men camped on Dean channel were terrified by a pack of these creatures prowling near at hand. Professor Boas gives accounts of this being on pages 83-90 of his report.

Gum or spruce gum is called snanic, which may be the same as sniniq. The sniniq, according to one of Professor Boas's accounts, made a great noise chewing gum.

HARLAN I. SMITH.

Africa, East: Ethnography.

Ingrams.

The People of Makunduchi, Zanzibar. By W. H. Ingrams.

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The aboriginal inhabitants of Zanzibar Island are known as the Hadimu, and little has been written about them, though their customs are interesting and repay study.

* May 31, 1923.

In the South-east of the Island is a large village called Makunduchi, the population of which is, according to the Native census of 1924, 1,190 adult males, 1,531 adult females, 615 boys and 575 girls,—total 3,911. The number of huts is 1,579. The name Makunduchi is derived by the inhabitants from the name of a village on the mainland, opposite the South part of the Island, called Konduchi, whence they state they came.

North of Makunduchi is another village called Jembiyani: this village shares with Makunduchi a peculiar dialect of Kihadimu.

Legend has it that many years ago an Arab visited the shore, before there were settlers there, and while waiting for his slave took off his Jambiya, or curved dagger, which every Arab wears fastened to a belt round the waist, and rested. When he went he forgot the dagger, which was subsequently found by a native coming to the shore to get firewood.

Not knowing what it was he went back to his village in the interior and told his people. An old man recognised it as an Arab Jambiya and it was left where it was on the shore, to avoid any charge of theft being made should the Arab return. He never did return, and for years and years the Jambiya was left there, the spot being always kept clear of bush and grass. The Jambiya finally perished entirely, but the spot is still kept clear and the village known as Jembiyani, or the "place of the dagger."

The population of Jembiyani is 465 males, 565 females, 238 boys, 218 girls—total 1,486. The number of huts is 593.

These villages are of exceptional interest, not only on account of the dialect but because certain other curious customs are observed there which are known to no other part of the Sultanate of Zanzibar. The people are Wahadimu, but different in many ways to the rest of that tribe. They are remarkably law-abiding and many display unusual intelligence.

Makunduchi is the only place in the Sultanate where transport is carried on with camels. These are mainly the property of the Indians, who are known as Makumbaro. With the exception of some in Zanzibar city, Makunduchi is the only place with a settlement of these Indians, whose home is in Cutch and Kathiawar. The word is derived from the Sanskrit Kumbhakar, a potter, and there are about thirty families of them settled there. They live in great friendliness with the Wahadimu, are very obliging people and bring up their families in the country. The children speak Kihadimu as fluently as the natives.

The women of Makunduchi and Jembiyani make coloured mats with a fringe, from the fronds of the wild date palm. This fringe is called *ndevu*, or beard, and is not made on the mats of other villages.

I now turn to a description of the customs which make Makunduchi so different from any other place in Zanzibar.

The dance Nyange.—This exorcising dance is played by women, the orchestra only consisting of men. The instruments used are a drum on three legs called *mrungura*, a small barrel drum called *chapuo*, a brass gong (*upatu*), the native clarionet (*zomuri*) and another three-legged but bottomless drum called *mshindo*. Under a *makuti* (palm leaf) shelter crowds of women dance backwards and forwards. Under the shelter and at one side is a small tent, made of the coloured cloths the women wear, in which sits the woman possessed of the devil the dancers seek to exorcise.

Every now and then the women dance out at one end of the shelter into the sunlight to fall back again together into the shelter. The movement is extraordinarily reminiscent of the waves on a sandy shore, as they dash up, break into foam and catch the sunlight to fall back again into the unbroken sea.

About a dozen of the women carry iron tridents* affixed to long handles, one or two have swords, others ivory-handled knives, a few carry model *ngalawas* (outrigger canoes) and some model paddles, in size too big in proportion for the canoes. Canoes and paddles are painted red, with wavy white lines on them.

In Zanzibar dances special food has to be provided for each devil; the appropriate nourishment in this case consists of dates, cardamom, raisins, cinnamon, white bread (called at Makunduchi *pufudo*), pumpkins and pomegranates.

The devil cannot understand the local dialect (this is a common feature of Zanzibar devils, who speak a variety of tongues known collectively as *Kipepo*) and can only make its wants felt in such incomprehensible syllables as :—

Zijejera zizejedata

Nzujajajajira

The former is said to denote a desire for food and the latter for water.

The devil Nyange is said to be contracted only at sea, and the result of enquiries as to its origin was that many years ago three women went down to the shore to catch *kidagaa* ("whitebait"). There they saw a devil riding towards them in a canoe and holding in his hand a trident. At the sight of him they were afflicted with madness. No one knew how the devil, stranger as he was to their pantheon, could be dealt with, but the remedy was revealed in the night in a dream and the dance Nyange is the result.

Apart from this dance being an interesting example of homoeopathic magic, no one can fail to be struck by the likeness of the sea-devil with a trident (a weapon unknown in Zanzibar and used by the people of Makunduchi only in this dance) to the story of Poseidon. It is just possible that there is more in it than coincidence.

It is well known that the Greeks knew and travelled along the East coast of Africa. It is from a Greek book we get our first description of Pemba, called Menuthias, and mention is made of Rhapta, "the very last market town of the continent of Azania." Pangani has been suggested as a site of this lost town,† but the discovery of a coin of Ptolemy X Soter (115-80 B.C.) at Msasani, north of Dar-es-Salaam suggests another situation.‡ The description of Msasani Bay and Harbour in "The African Pilot" shows it to be just such a place as would have been a pleasing harbour to the ancients. Msasani is but a short distance from Konduchi, and it is extremely probable that at the last town on the coast, on some suitable promontory, the Greek sailors would have erected a temple to the presiding god of the sea, in his capacity of Soter, the saviour, to whom they owed their safety for their voyage outwards and to whom they would pray for a safe journey home. These jealous gods were of the same nature as the spirits of the Africans, and one can easily imagine that to a simple fisher folk the worship of Poseidon would make a strong appeal.

The madness caused by the sight of the devil is reminiscent of Pan, but I am unaware if similar madness was caused by the sight of the other nature gods.

Pungwa and *Shomoo*.—“*Pungwa*” is the passive form of “*Punga*” and “*Kupunga pepo*” means to exorcise a devil.

Owing to the activities of witch doctors and the evil they may do and plant about the country for the unsuspecting traveller, the people of Makunduchi have evolved a guild of devil hunters.

Prior to the hunt Pungwa is played all night to get the “pack” into the proper psychic state to enable them to trace the evil spirits left about by less scrupulous people. Men only take part in the ceremony, which is performed in much secrecy, to the accompaniment of two drums (*mshindo* and *chapuo*).

* One was exhibited in the Zanzibar Court of the East Africa Pavilion at the British Empire Exhibition, 1924.

† W. Schoff, “Periplus of the Erythræan Sea” (Longmans, Green & Co.).

‡ F. Stuhlmann, “Beiträge zur Kulturgeschichte von Ost Afrika.” Berlin, 1909.

In the morning the *Shomoo* takes place. Carrying such exorcising implements as a *Pini* (a cow's horn, with two bells attached, and stuffed with roots and a dog's nose, as a dog is a good tracker) and *Usinga* (a cow's tail affixed to a stick), the devil hunters search all over the countryside, now running about, now crawling, now crouching, but always together. When they get a scent they follow it up. Digging with their hands, smelling with their noses, they finally unearth charms buried by the witch doctors in which a *Mzuka* (evil spirit) is imprisoned to do the passer-by harm. A man accompanies them with a bucket full of herbs and water, of which the hunters partake or with which they are sprinkled.

The Wamavua.—The wamavua of Makunduchi have now practically died out. At their most they consisted of about 25 individuals, members of a few families who were known by that name and followed the cult of rain spirits of the same name.

Their habits were peculiar : they wore no headdress, never slept on beds and had a white flag tied to the outside of their houses. They originally only married among themselves and only the eldest son or daughter belonged to the guild.

Their place of worship was a cave, called "genge la mavua," to which they went to intercede with the rain spirits.

They entered the cave on hands and knees, clad in a short white kanzu only, kneeling or prostrating themselves round a small circle of stones and fragments, in the middle of which was a stick to which white cloth was tied from time to time. They burnt incense on a small fire of coconut shells and prayed to the spirits for rain or to avert sickness. Their food consisted chiefly of "chuwali," a mollusc common on the shore and which appears to be a species of *murex*.

Their cave was always kept swept and tidy, and, though deserted for many years, was still tidy when I visited it, though the white cloth had almost entirely rotted away with age.

The best known of them was an old lady named Mame Hodi, whose grandson, Karibu bin Hodi, and another ex-member of the wamavua, informed me they had seen her walk out over the sea, where she remained for three days. She is also remembered as a teller of the future and said to have foretold many events, including an outbreak of smallpox. She never failed to obtain rain.

The wamavua spirits which, unlike many Zanzibar devils, can travel by sea, were for that reason in all probability, like the Djinn, imported from some other country. They are now said to have left, so their worship has broken up.

Naoruz or Siku ya Mwaka.—The aborigines of Zanzibar and Pemba retain the old Solar year inherited from the Persians, but in addition to the customs observed on New Year's day by the rest of the Wahadimu, Watumbatu and Wapemba, which include extinguishing the fires, depositing the ashes at the cross roads, sprinkling the floury ash against the back of the houses, bathing in the sea and ceremonial feasts, the people of Makunduchi have two other customs. They build a small hut or banda of dried coconut leaves and put two people inside. They then set fire to the hut and throw stones into the flames. The two men are supposed to remain inside but in reality escape unseen through the back of the hut.

After this the old men dance round the grave of a bygone patriarch whose name is forgotten but who, like Mame Hodi mentioned above, could walk the waves at will.

Dialect.—It is impossible in this paper to give a vocabulary of the dialect of Makunduchi and Jembiyani, and in any case it is advisable that it should be given with the other Zanzibar and Pemba dialects for the sake of comparison.

The conjugations of the verb are, however, so interesting that some specimens may perhaps be given here :—

Verb—To know.

Swahili.	Makunduchi.
kujuua.	kuvijua.

Present.		Future.		Past.	
Swahili.	Makunduchi.	Swahili.	Makunduchi.	Swahili.	Makunduchi.
Najua	Mvijua	Nitajua	Chavijua	Nalijua	Nyevunyivijua
Unajua	Kuvijua	Utajua	Kuchavijua	Ulijua	Kuevukuvijua
Anajua	Avijua	Atajua	Kachavijua	Alijua	Kevukavijua
Tunajua	Tuvijua	Tutajua	Tuchavijua	Tulijua	Twevutuvijua
Mnajua	Mvijua	Mtajua	Mchavijua	Mlijua	Mvumvijua
Wanajua	Wavijua	Watajua	Wachavijua	Walijua	Wevuavijua

Perfect.—Same as present, but 3rd singular is kavijua.

Present negative.

Swahili.	Makunduchi.
Sijue	Sivije
Hujue, etc.	Huvije, etc.

W. H. INGRAMS.

Asia, Western : Magic.

Smith.

The Pomegranate as a Charm. *By Sidney Smith*

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That the forms of beads depend upon religious and magical beliefs is a generally accepted opinion, yet the exact significance can rarely be ascertained when ancient objects are concerned. It is, therefore, of interest to note the true significance of a type of bead very common in Western Asia and Egypt in ancient times—the pomegranate. Beads of this shape in gold and paste may be seen in Cases C and D of Room IV of the Semitic Galleries, and in the Egyptian Galleries in the British Museum. Mr. Campbell Thompson has conclusively proved (*Assyrian Herbal*, 281-2) that the pomegranate was called in Akkadian (Babylonian) *nurmu*, and that these beads were intended to represent that fruit.

Now Assyrian texts have been published (by Ebeling, *Keilschrifttexte aus Assur religiösen Inhalten*, II, 61 and 69) which have direct reference to the pomegranate, a fact not apprehended by the translator (Ebeling, *Liebeszauber im alten Orient*). One passage reads: “(Incantation.) A maid, beautiful, loving, has come forth. “The goddess Ishtar, who loveth the apple and pomegranate, sexual strength, “has come forth. . . . (Prayer for ‘If a woman has raised her eye to a man’s “penis.’) (Ritual for it.) Recite the incantation three times over either an “apple or a pomegranate. Give it to the woman. Make her suck its juice. That “woman will come and he will love her.”

The use of the pomegranate as an aphrodisiac in this type of magic is readily intelligible, and should explain the popularity of the pomegranate-shaped bead. The fruit was obviously considered as effective as a charm as when used for love potions. The bead in the shape of a pomegranate may have had, for instance, the same significance for the Greeks, to whom it was known (see the Room of Greek and Roman Life, Case L). Mr. E. J. Forsdyke informs me that the pomegranate in classical times was a common symbol of fertility.

SIDNEY SMITH.

REVIEWS.

Psycho-Analysis.

Freud.

Collected Papers, by Sigm. Freud., M.D., LL.D. 4 volumes. London, 1924 and 1925. The Hogarth Press and the Institute of Psycho-analysis.

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Although such of Freud's works as have been published in book form have for the most part been available in English translations for a number of years, the great majority of his shorter writings could up to the present be consulted only in German periodicals, which were somewhat inaccessible to English readers. The present collection of Freud's scattered papers in an excellent translation will,

therefore, be a matter for rejoicing on the part of all English-speaking students of psycho-analysis in any of its aspects or applications. The appearance of this collection will be welcomed not only for the sake of convenience, but also (and chiefly) because it contains a very large proportion of the empirical data which must be considered in any attempt at estimating the scientific status and value of psycho-analysis—both as therapeutic method and as psychological theory. As Dr. Ernest Jones, the general editor of the collection (which forms part of the International Psycho-analytical Library), says in his preface to the first volume, the real basis of psycho-analysis is to be found in Freud's clinical studies, and the books which have hitherto been most familiar in this country represent something in the nature of superstructure, in the form of applications of the conclusions reached through these studies to the special problems of dreams, sex, totemism, etc. Psycho-analysis originated as a clinical method, and it was as a result of certain intensive studies of the diseased mind that there were evolved those views which have since shown themselves capable of application to the most varied problems of psychology, aesthetics, sociology and anthropology.

This further application over a wide field of what was in the beginning merely an innovation in medical technique gives rise to one of the most interesting problems of present-day comparative science—and this whatever be the eventual outcome of the application. If, as some think, the whole psycho-analytical theory is riddled with fallacies, the widespread attempt to interpret fresh phenomena in the light of views based on faulty inference or biased observation will surely come to be regarded as an *abschreckendes Beispiel* which workers in all the fields affected will do well to ponder in order to prevent the occurrence of similar scientific lapses in the future. If, on the other hand, the application of psycho-analytic principles is valid and the interpretations gained thereby are in the main correct, it is difficult to avoid the conclusion that the work of Freud constitutes one of the most significant advances in the history of science as applied to human life and human institutions. In either case most important methodological questions are involved. In the first place are Freud's original clinical observations sound enough to inspire confidence in his conclusions and in the application of his views and methods to further fields? In the second place, does this further application result in such additional insight and supply fresh data of such a kind as to justify us in holding that the views of psycho-analysts—originally evolved in the field of abnormal psychology—have received corroborative support from independent sources? This second problem can only be answered by specialists within the fields in which the applications of psycho-analytic principles are made. The first question can to some extent be answered by a careful study of the present books.

At the moment of writing, three of the four volumes have been published. These volumes deal principally with clinical material, the earlier attempts at theoretical construction and questions of technique. The fourth volume is to contain the later, more ambitious, efforts at theory—a line of work that Freud has designated Metapsychology—and some papers dealing with the application of psycho-analysis to non-medical topics. An excellent feature of the present collection is that the reader's attention is forcibly drawn to the date of each paper, which is printed immediately under the title and also at the top of each page. As the editor very rightly says, "the only really satisfactory way of acquiring a 'knowledge of Prof. Freud's writings is to follow the development of his work,'" i.e., to read each paper with careful reference to its place in the chronological sequence. So important indeed is this point that it would perhaps have been preferable in many ways to have adopted quite frankly a chronological arrangement instead of the not entirely satisfactory or consistent classification under the headings "Early Papers," "Clinical Papers," "Papers on Technique" and "Case Histories."

In the case of the collected works of a living writer, the chronological arrangement has also the advantage of allowing future contributions to be incorporated in the series without disturbance of the general plan.

The translation of the papers contained in the first two volumes has been carried out by various hands under the supervision of Mrs. Joan Riviere, while Mr. and Mrs. James Strachey are responsible for the translation of the whole of the third volume. All the work of translation has been carefully done and represents a great improvement on some of the earlier versions of Freud's works in the English tongue.

J. C. FLÜGEL.

Congo: Ethnography.

Torday.

On the Trail of the Bushongo. By E. Torday. London: Seeley, Service & Co. 1925. Pp. 286, with 59 illustrations and a map. Price 25s. nett. **89**

Mr. Torday has written a popular account of his work in the Congo, most of which has already appeared in scientific form in joint authorship with Mr. T. A. Joyce. The book is full of interest, and the personal contact with the natives, which is vividly recounted, is a great help towards the understanding of these people.

Mr. Torday is able to bring together considerable evidence pointing to the region of Lake Chad as the original home of the Bushongo, or rather of the Bambala, their paramount tribe. He suggests that when the Bambala—or the Abira as they were probably called (Bambala being a Bantuised form)—settled in their present home they gave up their patrilineal institutions and adopted the matrilineal succession and inheritance of the Bantu people whom they had conquered. Could this conjecture be substantiated, it would be an example of considerable sociological importance. The change, however, is not quite so striking as it seems at first, for the *ikina bari* or totem taboo pass in the male line, that is to say a man belongs to the clan of his father, observes its taboo and must not marry a woman having the same *ikina bari*. It is well known that clan descent, inheritance and succession do not necessarily follow the same rules in any single people; but the reverse condition with clan descent in the female line and inheritance or succession in the male line, is so much more common, that the case of the Bambala deserves attention. Among the Bushongo we have clan descent patrilineal, and habitation patrilocal, but succession to the throne, to all office and to chieftainship, as well as the inheritance of property, are matrilineal. It would seem, however, that the bride-price is paid to the father.

A curious feature in the royal succession is possibly significant. When the King is about to die, he sends for his eldest son and tells him who is to be his heir, for the King is at liberty to chose his successor from among his maternal relatives. After the death of the King the government passes into the hands of the eldest son, who is responsible for guarding the royal treasury and choosing the victims to be immolated on his father's grave. For three days the reign of the King's son lasts, while his father's body remains exposed; then, after the funeral, he publicly declares the name of the heir to the throne. Is this custom to be looked upon as evidence of previous patrilineal succession? It certainly must be regarded as evidence of the intimate relationship between father and son, but this may be only religious. Unfortunately, we do not know what part, if any, the son of a commoner takes at his father's funeral, nor do we know if the reincarnation of spirits, which is supposed to take place, follows any rule of inheritance, either matrilineal or patrilineal. It is difficult to understand why a patrilineal people who were able to establish their supremacy should have changed their rules of succession out of deference to the people they had conquered. Such a change might be expected to lead to quarrels in the royal family, unless, at one time,

the King's sons were killed and accompanied their royal father to the spirit world. A change in the mode of inheriting personal property is less difficult to imagine in this case. The invaders introduced a new weapon (the throwing knife), which was later abolished, but there is no tradition of their bringing cattle, nor is it likely that they could have passed through the great forest belt with cattle. So they probably had little to leave to their own sons and, by adopting the local mode of inheritance, were actually benefitting their children, who would thus inherit when the conquerors had married indigenous women. An examination of the somewhat incomplete list of the kinship terms of the Bambala given in *Notes Ethnographiques* supports the view that the kinship system was reckoned in the male line. However, the evidence for the change in succession and inheritance, though suggestive, is insufficient and by no means conclusive.

In this book Mr. Torday has published some notes on the Baluba which he had not before recorded. These, though fragmentary, are interesting.

BRENDA Z. SELIGMAN.

New Zealand: Ethnology.

Best.

The Maori. By Elsdon Best. Vol. I. Wellington, N.Z. 1924. **90**
Dominion Museum, Bull. No. 10, pp. 264.

This work, which has been printed by the newly-formed Board of Maori Ethnological Research, is by Mr. Elsdon Best, F.N.Z.Inst., who is well known as an authority on anything connected with the Maori, whose customs and traditions he has studied closely for half a century.

Mr. Best has published many valuable papers dealing with branches of his subject, and the present work appears to aim at making the main results of his researches handily accessible to a wider circle of readers. He has here departed from his usual custom of recording only the results of his own personal observations, and includes some matter on the authority of others, some of whom have not always been as cautious as Mr. Best himself.

In a short chapter on Physical and Mental Characteristics, it is pointed out that the Maori race shows traces of two main stocks, the more important being Polynesian, the other having Melanesian characteristics. The latter may represent pre-Maori inhabitants, or may be due to an admixture from castaways of later date. Possibly both factors are involved; but either introduces the difficulty of the inferior seamanship of the Melanesian.

Great stress is rightly laid upon the prowess of the early Polynesian as a navigator. His daring and ability compel our highest admiration.

The most important part of the book is that which deals with the Philosophy of the Maori, if we may so style it, and his Myths, Folklore, and Religious Beliefs, which were closely allied to it. It is shown that all these matters were most carefully preserved and the traditions faithfully handed on to chosen pupils, who were destined in their turn to become the recognised authorities and repositories of learning. It is in this department that the greatest difficulties occur. Mr. Best points out that there are many inconsistencies and apparent contradictions; and there are others to which he does not call attention. These may, in part, be due to local differences having developed in the tradition, or they may sometimes be due to mistakes having been made by previous observers. But, undoubtedly, we have it disclosed that there were two distinct strata of learning, each with its accompanying religious beliefs: that of the common people, which rose no higher than supernatural beings presiding over the forces of nature; and that of the instructed few, who recognised a supreme being Io, who was over all minor deities and the whole universe. This esoteric knowledge has only come to light in recent years, and even now our sources of information require further sifting. Best claims

for the Maori that he had a conception of this Io as one wholly removed from everything evil; that, in fact, the *Makutu*, or witchcraft, which was an ever-present terror to the Maori, had no contact with Io, but had to be taught in a separate school. But the authorities he quotes represent *Tane* as bringing down to man three "baskets of knowledge" from Io, and one of these was that of *Makutu*. Also, it is stated that this branch of knowledge was one of those offered to the pupil on his entry to the sacred school.

The fate of the spirits of the deceased introduces other difficulties, and Best is probably right in the suspicion with which he views the statement that there was discrimination between the virtuous and the wicked. This and one or two other details suggest unconscious assimilation of foreign teaching, a disturbing element which must be carefully watched for.

Mr. Best warns his readers against supposing that *tohunga* is accurately represented by "priest," or *tapu* by "holy"; but he finds it difficult to avoid the use of these imperfect renderings, and uses others, such as "altar" and "sacrifice," which may suggest wrong ideas to the English reader.

The last two chapters are devoted to Social Customs, including rank, relationship, form of address, nomenclature of persons and places, cooking and preparation of food, marriage, and similar topics.

The book contains a number of illustrations, most of them germane to the text. There is a very necessary index; but a glossary of the Maori terms used would be a valuable addition, and might with advantage be introduced in the concluding volume of the series.

The Board of Ethnological Research is to be congratulated on bringing out a book which is certain to be recognised as an authoritative compendium of information on the Maori.

HERBERT W. WILLIAMS.

Spain : Archæology.

Obermaier : Pittard.

Fossil Man in Spain. By Prof. Hugo Obermaier. Published for the Hispanic Society of America. London : Oxford University Press. 1924. **91**
Pp. 464. $9\frac{1}{2} \times 6\frac{1}{2}$. Price 23s. net.

Les Races et l'Histoire. By Prof. Pittard of Geneva. *L'Évolution de l'Humanité* series. Paris : La Renaissance du Livre. 20 frs. net.

The Hispanic Society of America is to be congratulated on having brought out an English edition of "El Hombre Fosil." The author, Dr. Obermaier, now Professor of Prehistoric Archæology at the University of Madrid, is in the very foremost rank of prehistorians and quaternary geologists, and though his illustrations and sites are mainly drawn from the Spanish Peninsula, the book is really a first-class general account of our knowledge of Palæolithic man and his surroundings. The Spanish edition, which appeared in 1916, has always been a *sine qua non* to specialists, and it is now good to have the present volume, which will be more acceptable to the general reader from the language point of view. There has been a certain amount of revision, and the latest finds, *e.g.*, plates of the art discoveries near Castileon and of the painting of a man up a sort of rope ladder endeavouring to take honey while surrounded by the angry bees, have been introduced. One also welcomes the addition of a photograph showing a view of the cave of Castillo (Puente Viesgo, N. Spain), which was dug by Dr. Obermaier and proved to be the most important prehistoric excavation ever made. It is to be regretted that the war has hitherto prevented the complete publication of this site. A general account is given in the present volume, but necessarily details are lacking as further comparative study of the materials collected has still to be made.

The contents of the book are briefly as follows: A discussion of the problems of Tertiary man; followed by chapters on the Glacial Epoch with lists of flora and fauna. In this connection, Dr. Obermaier is very much at home having been a pupil of Penck, although he no longer exactly follows the latter's system of chronology. Next, the successive early cultures are passed in review, a special chapter being allotted to the Spanish Peninsula at this time. Then chapters on Palæolithic Art, the problems of chronology, Fossil Man, and the transition from Palæolithic to Neolithic. Finally, the book concludes with full and valuable appendices and bibliographies.

Besides the importance of the text one is impressed by the profusion and beauty of the illustrations. A volume such as this, richly got up on excellent paper and sold at the very reasonable figure of 23s., may well make us slightly envy the enterprise of our American cousins. To the student of prehistory no better book can be recommended, and Dr. Obermaier and the translator are to be highly congratulated.

The reader will not find "Les Races et l'Histoire," by Prof. Pittard of Geneva, in the least duplicating such English works as Dr. Haddon's "Races of Man," etc.; the objective is quite different. Whereas most anthropologists, after defining their method of classification, are concerned with dividing the world's inhabitants into various groups of peoples with perhaps an account of their major migrations and wanderings, Prof. Pittard is mainly interested in the influences these groups have had upon history. In both cases there is an account of the method of classification—hair, pigmentation, stature, etc.—but whereas in the one case the various races all over the world are of equal importance and Oceania is as carefully discussed as Europe, in the other case Europe with its longer known and, to us at any rate, more important history has the lion's share. The result is an interesting work of some 600 pages and, as is the case with all the volumes in the *Évolution de l'Humanité* series, it is not only written for the scientific specialist but, the style and language being lucid and clear, can be enjoyed also by the amateur.

Considering the world geographically, Prof. Pittard gives an account of the various peoples who have occupied each country from prehistoric times onwards, and of how they have affected its history. That the different races of mankind do respond differently when they have migrated into a similar environment is no doubt a truism, but it is interesting to see how the story is worked out by the great Swiss savant. One is perhaps just a little sorry for his diffidence in giving us so many notes of interrogation; it is sometimes better to take the plunge as, if necessary, one can always be honest and publicly recross the ford in a subsequent edition. Still, the number of fascinating questions he has put before us, even if the solutions are not as yet always attainable, gives rise to much thought, emphasises the importance of the subject, and shows how neither the historian nor the statesman interested in the future of his race or country can afford to neglect the anthropologist.

M. C. BURKITT.

India : Religion.

Griswold.

The Religious Quest of India. The Religion of the Rigveda. By H. D. Griswold, Ph.D., D.D. Humphrey Milford, Oxford University Press, 1923. **92**
Pp. 375. $8\frac{3}{4} \times 5\frac{3}{4}$. Price 12s. 6d. net.

"Like the gleaming of the Himalayan snows to the traveller on the plains
" is the morning beauty of the Rigveda, more attractive, if possible, to our hearts
" than the dazzling splendour of the Homeric poems. It is the whole of the
" picture, the people, the poetry and the faith reflected in it—that captivates our
" hearts." These words, which occur in the final chapter of Dr. Griswold's work,
give the clue to the spirit which animates it. For, apart from the scholastic value

of the book, apart from the knowledge which it displays, what strikes one most forcibly is the extreme *sympathy* with which the author has analysed and portrayed the religious ideas of the Rigveda. To use his own phrase, he looks upon the Rigveda as "the purest expression on the soil of India of the Aryan spirit," and it is almost with a feeling of brotherhood for the Vedic Indians on "that far off "bank and shoal of time," that he addresses himself to the task of elucidating the religious conceptions and ideals enshrined in the famous hymns.

In the opening chapter on the Antecedents of the Rigvedic Age, Dr. Griswold distinguishes between the Indo-European period, when the clans had not yet separated, and the Indo-Iranian period, when the dispersion of the Indo-Europeans had taken place and the Indo-Iranian tribes were still living together as one people, possibly in Bactria and the neighbouring regions. In enumerating the customs which can be shown with tolerable certainty to have prevailed among the undivided Indo-European clans, he postulates the double form of religion, viz., the worship of ancestors and the worship of "the Heavenly Ones" or the personified phenomena of Nature. These two lines of religious development continued throughout the succeeding Indo-Iranian period, but assumed certain new and important features, which may be summarised as (a) the conception of "order" (*rita-asha*) ; (b) a movement towards ethical monotheism in the conception of the Vedic Varuna and the pre-Zoroastrian Ahura Mazda; (c) the appearance of groups of gods like the Ādityas and Amesha Spentas; (d) the growth of demonology and (e) the rise of the priesthood.

At this point we pass to the Rigvedic Age itself, concerning which the author gives a succinct and interesting review of the geographical, climatic, social and political circumstances of the Indo-Aryans—the Vedic Aryans, as he terms them—who, after their separation from their Iranian fellow-tribesmen, occupied the area which roughly corresponds in these days to the Panjab, the North-West Frontier Province, and the Kabul Valley. Dr. Griswold, arguing on the analogy of other early racial expansions, is of opinion that there was a series of Aryan invasions and settlements in the Panjab extending over several centuries, possibly from 1700 to 1300 B.C., rather than one single invasion and conquest; and the history of the gradual acquisition of important portions of his own fatherland by the backwoodsmen and other American pioneers appears to support his views regarding the conquest of the Panjab from the aboriginal Dasyus.

The third chapter, which is also introductory to the main theme of Rigvedic religion, is concerned with the text, language, chronology and interpretation of the Rigveda, and contains a *résumé* of the views of the leading scholars on controversial points, with Dr. Griswold's own conclusions thereon. In the matter of the interpretation of the Rigveda it is interesting to remark the importance which he attaches to the study of modern religious customs and folklore, pointing out that among the authoritative works included at the end of Professor Hopkins's *Vedic Religion of India*, Crooke's *Popular Religion and Folklore of Northern India* occupies the first place. There is little doubt, indeed, that modern Hindu ritual is the result of the fusion of two streams, the Aryan, going back to the Rigveda, and the Dravidian, representing primitive indigenous custom; while, as Dr. Griswold states, the large magical element in the popular religion of modern India is unquestionably derived from Aryan, that is to say, from Rigvedic, as well as from Dravidian sources.

A discussion of the Vedic world of gods and demons, which expounds the character of their joint functioning and their connection with *Rita* or eternal order, leads to an illuminating analysis of the character of Varuna, "the most impressive "deity among all the Vedic gods." A perusal of Dr. Griswold's facts and arguments cannot but command acceptance of his view that the Vedic Indians

were not without "the conception of a law of righteousness viewed as the will of a "holy God"; but he issues a note of warning at the same time against the fallacy of importing modern and especially Christian feelings and conceptions into the hymns. There is a natural disposition to do so, when one meets with a deity so ethically exalted as the Vedic Varuna, and when one finds that there is hardly a hymn addressed to him which does not contain some allusion to sin and its remission. In dealing with the Adityas and Amesha Spentas, Dr. Griswold discusses Oldenburg's hypothesis of Semitic influence on the conception of these groups of bright deities; and, while not prepared to accept Oldenburg's theory as it stands, he suggests that there may have been "an influence, if not direct, at least indirect, subtle, and almost telepathic, over the religious thinking of the undivided Indo-Iranian people in Bactria on the part of the culturally and religiously more developed Babylonians." The Indo-Iranians had their own lofty conception of Dyaus as Father and Lord, and the great ethical conception of *Asharita*, "order, righteousness, truth." Possibly, as Dr. Griswold remarks, these ideas fructified under the influence of Babylonia, between whom and Bactria there may quite possibly have been direct intercourse during the period of Indo-Iranian unity (B.C. 2500-1500 *circa*).

Space does not permit of a detailed reference to the chapters on Agni, the ritualistic god, and Indra, "the apotheosis of naked might, the embodiment of the martial and imperialistic tendencies of the Vedic Indians." The extraordinary similarity between the development of the Rigvedic storm-god and that of the Hebrew Yahweh must strike everyone who reads the exploits of Indra in the Rigveda and the acts attributed to the god of the Israelites in the 136th Psalm. The analysis of *Soma*, the deified sacrificial drink, leads to a triple distinction between *Soma* the plant, *Soma* the drink, and *Soma* the heavenly nectar, of which the earthly *Soma* is both the symbol and embodiment, and invites a comparison with the triple Biblical conception of the Tree of Life, the water of Life, and the Holy Spirit. Although there are practically no traces of a pessimistic attitude toward life in the Rigveda, we do find a pensive note in some of the later hymns to Ushas (the Dawn), "which, while in no sense abnormal, may perhaps be regarded as pointing in the direction of the later pessimism": while the idea of Transmigration may be described as faintly shadowed in the repeated birth of the Dawn-goddess. But Hindu pessimism is, when all is said, wholly foreign to the spirit of the Rigvedic hymns, and probably originated, according to the author's view, partly in the trying climate of India and the gradual fusion of the Aryans with the aborigines, and partly in the doctrines of *Karma* and transmigration, which tend to produce in many minds a spirit of hopelessness.

Dr. Griswold has a good chapter on the minor Vedic gods, Surya, Savitar (the morning and evening glow), Pūṣan (the sun as a pastoral deity or heavenly herdsman), Vishnu (the personified swiftly-moving luminary), Vayu (the wind), Āpah (the waters, source of healing and immortality) and Rudra (the destructive agency of the lightning). "The Eschatology of the Rigveda" is a suggestive chapter, which will repay perusal, as also will that on "The Rigveda and later Hindu Developments." For it is perfectly true, in the words of Dr. Griswold, that it is no more possible to understand later Hinduism without a knowledge of the Rigveda than it would be to understand the New Testament or the Qurān without a knowledge of the Old Testament. In some ways the last chapter on "The Fulfilment of the Religion of the Rigveda" is the best of all, with its parallels or points of contact between Rigvedic and Hebrew and Christian doctrine, its discussion of the Theism founded on the Rigveda by Svāmī Dayānand Sarasvati, and its final cry of regret that the supremely noble figure of Varuna, the Rigvedic Creator and God of Righteousness, should have degenerated before the end of the

Vedic period into a petty godling of the waters. It is evident that Dr Griswold feels deeply the tragedy of Varuna's fall, which deprived India entirely of "all the priceless promise of that early faith," and as a Christian and a missionary he can only point to the figure whom he himself worships, as the ultimate and the noblest substitute for the supreme deity of the Rigvedic age. The Oxford University Press is to be congratulated upon a worthy addition to its series of publications dealing with India.

S. M. E.

CORRESPONDENCE.

Religion.

Rose

The Polite Plural Again.*To the Editor of MAN.*

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SIR,—Mr. Hocart is mistaken in supposing that I had not seen his letter (MAN, 1924, 62) before adding my contribution to the above discussion. It was the combination of that with his article on "The King's Justice" (1924, 53) which suggested to me that he thought the Latin and Greek use of the plural of courtesy might be connected with local ideas of the divinity of kings. However, nothing could be clearer than his present statement of his position (1924, 128). The polite plural, he holds, originated in India, "any time B.C.," and the Romans obtained it from those countries which the Arabs afterwards invaded—*i.e.*, the Near East—perhaps having no idea of its original significance.

Now we know pretty definitely the source of Roman ideas as regards court etiquette (to which the Latin plural of courtesy seems to belong) in the days when the East influenced them most—viz., the third and fourth centuries. They drew upon Persian ceremonial, that of the revived Persian Empire under Shapur or Sapor I. That the influence of Persia was considerable, and not confined to this one sphere, is very plain; I need only mention the spread of Mithraism over the whole Roman world as an instance of it.

If we assume the Persian Empire, then, as the source of the late Latin use of "you" for "thou" we postulate a *uera causa*, for,

(1) The Persians at that date, and not only at that date, believed in the divinity of their kings, at least officially. "I am the Mazdazasman" says Shapur I in an inscription; "Lord Shapur, the King of the Kings of Aîran and non-Aîran, of spiritual origin from the Deity."

(2) They used the polite plural in addressing their kings. This we find in a document which, in its present form, dates from about 600 A.D., the "Kârnâmak i Ardershîr i Pâpakan." We have, I am assured,* no reason to suppose that in matters of grammar it has been changed from its original form, which was written about 350 A.D.; therefore, as in it the King of Persia is regularly called "you," it may safely be assumed that he was so called in the times of Constantine and his immediate successors.

From Persia, therefore, Rome *might* have derived the polite plural, either alone or as part of the ritual for addressing a divine king.

But here the connecting-link fails us. If the Latin plural of courtesy is a Persian importation, we should surely have some hint in some Roman writer that it is Persian. But, on the contrary, we find a quite well-informed Roman showing that he supposes the Persians to have called kings "thou" in official language. I do not refer to the wretched scribbler to whom we owe the fragmentary "Life of Aurelian" in the "Historia Augusta," in which we have a series of letters, supposedly addressed to Sapor from vassal kings, all calling him "thou," but to the corre-

* All this Persian learning I owe to Mr. Ed. Edwards of the British Museum.

spondence between Constantius and Sapor in Ammianus Marcellinus, xvii, 5, 3 *sqq.* Here we have an author who had served against the Persians himself, and certainly knows something about them, and, while the style and tone of the letters show that he is not translating any Pahlevi document, he naturally, and following the practice of ancient historians, tries to make his monarchs write to each other as he conceives they really would write. Now in the letters in question "you" is never employed either by Sapor to Constantius or by Constantius to Sapor; but the latter says *ad optimatem uiam te reuertisse*, "that thou hast returned to the right way," and the former, *ut futurus tibi, si uelis, amicus* "as minded to be thy friend, if thou wilt have it so". Now, if Ammianus did not know that Persian court style involved the polite plural, its existence in Persian cannot have been common knowledge; and certainly the average uninformed person did not know it, as the passages from the "Historia Augusta" show (the same writer, "Hist. Aug." xxii, 6 (2), 7, makes Valerian call Decius "you"). But they could not copy an idiom of whose existence they were ignorant; therefore the Latin polite plural is not taken from Persian.

As to my own view, that the polite plural grew out of a combination of native idioms, I do not, as Mr. Hocart rightly says, explain why it did not develop until the fourth century, for the simple reason that I do not know. Neither do I know why accent, in early days the predominant feature of Latin verse, did not oust quantity till later still; nor why the use of *quod* to introduce an indirect statement, after showing its head as early as the time of Nero, took centuries to get a real footing in common usage; nor why a score of other phenomena which appear in early Latin disappear again till the late stages of the language. I have all Mr. Hocart's healthy contempt for "airy suggestions"; and until he can show some evidence to counter that which I have adduced to prove that the Romans (and the same seems true of Greeks) knew nothing of the existence of the Persian plural of courtesy, or can show that they got theirs from some other Eastern language, I am afraid I must put his theory into that category, so far as the ancient area of European culture is concerned.

Yours faithfully,

H. J. ROSE.

Britain: Archæology.

Garrod.

Solutrean Implements.

To the Editor of MAN.

SIR,—Mr. Reid Moir (MAN, 1925, 66) questions the statement made by Mr. Burkitt and myself that laurel-leaf blades are not confined to the Solutrean. He asks us

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(1) to define a laurel-leaf blade,

(2) to prove that such blades undoubtedly occur in other than Solutrean sites.

(1) A typical laurel-leaf may be defined as a stone-blade having more or less the outline of a laurel-leaf, reduced to leaf-like thinness by fine squamous flaking over the whole of both faces. In the Solutrean of Western Europe the laurel-leaf is reduced to its final shape by a minute retouch all round the edge on both faces, but in certain Central European sites, *e.g.*, Předmost, the edge-chipping is confined to one face.

(2) It is a well-known fact that flint blades conforming to the definition given above have been found in Late Neolithic and Early Bronze Age burials in Scandinavia, and in Early Bronze Age barrows in this country associated with beaker pottery, perforated axe-heads, tanged-and-barbed arrow-heads, jet buttons, etc. (R. A. Smith, *Proceedings of the Society of Antiquaries*, 27th November, 1919.) It is true that a certain number of the leaf-shaped blades from English barrows tend to be broadest towards the point and thickest towards the butt, and so

approach the typical Bronze Age flint dagger, while in some the flake-scars run in very regular parallel series unlike the Solutrean retouch, but even when these have been eliminated there remain a certain number which do not appear to be distinguishable from Solutrean forms. Mr. E. T. Leeds has a very fine laurel-leaf, found associated with a typical Early Bronze Age flint-dagger in a barrow at Herdman's Hill, Newark, near Peterborough. If this specimen had come from the surface or from an undated deposit I do not think Mr. Moir would have hesitated to describe it as Solutrean.

Having accepted Mr. Moir's challenge to the best of my ability, I think I have the right to ask him, in his turn, to define what he means by a laurel-leaf blade, and to tell Mr. Burkitt and myself by what criterion he would distinguish a tumulus specimen, such as that in the possession of Mr. Leeds, from an undoubted Solutrean laurel-leaf.

Yours faithfully,
D. A. E. GARROD.

ANTHROPOLOGICAL NOTES.

Lectures on Psycho-Analysis and Anthropology.—Dr. Geza Róheim, of Budapest, the author of a recently published work on Australian Totemism, of which a review will appear shortly in MAN, and a contributor both to MAN and the Journal of the Royal Anthropological Institute, will visit England in the month of September. During his stay he will deliver a course of four lectures for the Institute of Psycho-Analysis on Animism, Magic and other aspects of primitive religion. The fee for the course is one guinea. Further particulars may be obtained from Mr. John Rickman, Hon. Sec., 42, York Terrace, London, N.W.1.

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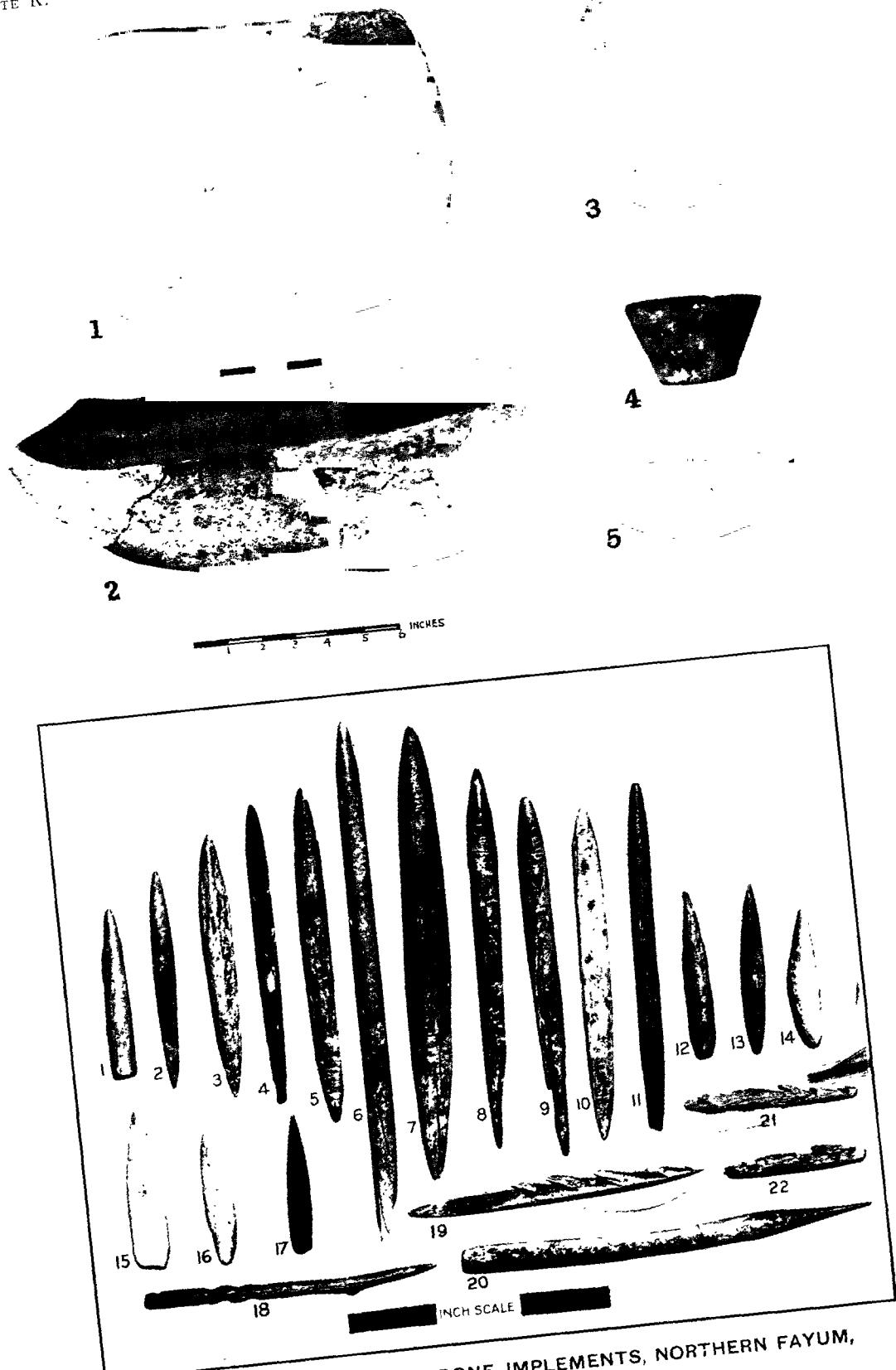
Dr. Róheim will also read a paper dealing with the Folk-lore and Primitive Beliefs of Hungary, at an evening meeting of the Royal Anthropological Institute.

A New German Anthropological Periodical.—The loss of her non-European colonies to Germany has greatly reduced the sum-total of opportunities and encouragement for field-work in anthropology. The several scientific expeditions organised annually by the Kolonialamt, the valuable yearly volumes of the *Mittheilungen aus den Deutscheschutzgebieten*, are no more. The publications and activities of the Hamburg Institute, of the Museums in the last-mentioned city, in Berlin, in Frankfurt, Cologne, Munich, Dresden have entirely ceased to exist, or lead a shadowy existence. The British anthropologist cannot help but feel some misgiving that there is no sign of the continuance of these activities, which cannot but be regarded as a duty, among the primitive peoples of the Mandated territories.

Germany, in spite of difficulties and disabilities, has produced some admirable research during and after the war, and it is a symptom of unabated energy that a new periodical has appeared, the *Zeitschrift für Völkerpsychologie*, edited by Professor Richard Thurnwald, the prominent German field anthropologist and a sociologist of great merit. This publication will fill a serious gap, as many German publications in anthropology and sociology have disappeared since the war or have been seriously reduced in size. Above all there is a great need for a periodical which will keep British and American anthropologists in touch with Continental work. The inclusion on the editorial staff of Dr. Malinowski, a Pole and Reader in Social Anthropology in the University of London, indicates that the aim of the new journal is truly international. In fact it is intimated that, should the numbers of English and American subscribers be sufficient, articles and reviews in English would be made a regular feature of the *Zeitschrift*.

MAN, OCTOBER, 1925.

PLATE K.



NEOLITHIC POTTERY AND BONE IMPLEMENTS, NORTHERN FAYUM,
EGYPT.

ORIGINAL ARTICLES.

With Plate K.

Egypt: Archæology.

Caton-Thompson.

**Preliminary Report on Neolithic Pottery and Bone Implements
from the Northern Fayum Desert, Egypt. By Gertrude Caton-
Thompson.**

In the course of investigations undertaken last winter by the British School of Archæology in Egypt, to inquire into the problems connected with the Fayum flint industry, the first pottery and bone implements associated with that culture were discovered. The area examined extended along the north shore of the Qarun Lake, from Khashmet ed Dib to a point some four miles south-west of Qasr el Sagra (Fig. 1), and many prehistoric settlements were found.

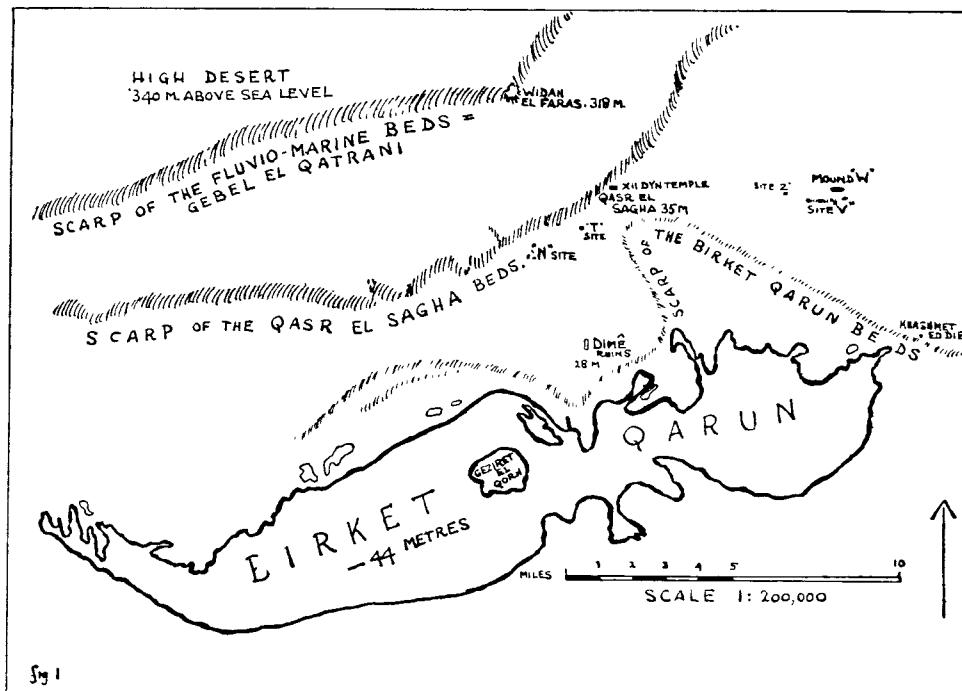


FIG. 1.—THE QARUN LAKE, NORTH FAYUM.

(a) THE POTTERY.—Although prehistoric sherds abounded on and around all settlement sites, it was extremely difficult to obtain a whole pot, or even enough of one to reconstruct the form. Many were practically powder, held together by salt; in such cases it was only possible to chisel away the salt as finely as possible, and to measure and draw the pot *in situ*, without attempt at removal. A few, however, were found in comparatively sound condition, and these are here figured and described.

Plate K. No. 1.—A typical Fayum cooking pot, hand-made and asymmetric. Internal diameter at rim, 23–26 cm.; circumference at greatest width, 96 cm.; height, 23 cm.; thickness of walls, about 0·6 cm. Flattened base. It is rough and badly fired, the insufficient heat applied resulting in an unintentional red and black mottling of the outer surface. Traces of a finer slip adhere in places, but the surface in the main is salt-corroded. The core of the biscuit is black—a further result of the badly regulated baking. Chopped straw is mixed with the clay, as in most rough prehistoric Egyptian pottery. The pot was found *in situ* at settlement

mound "W" at a depth of 8 inches, standing on its base in ashes and darkened sand, from which I sifted carbonised fragments of fish and small bird bones. Altitude over present lake level, 204 ft. = 60 ft. above sea-level.*

Plate K. No. 2.—An elongated shallow rectangular dish. Internal length, 36.5 cm.; internal width, 25.6 cm.; height, 9.4 cm.; thickness of walls, about 0.6 cm. The pottery is red, with some black external patches and core, and is of similar character and age to No. 1. No slip remains. This dish was found partially embedded in a weathered sandstone hillock some 12 ft. high (Fig. 2). Several such hillocks formed the centre of an important prehistoric camping ground, settlement site "N." The dish had, it would seem, been placed by a careful prehistoric owner out of harm's way, on a ledge in the sandstone, about 4 ft. above ground-level. Altitude over present lake, 195 ft. Many shreds of like pottery strewed the ground beneath, together with typical implements of the Fayum industry, and a few polished tools. The rapidly eroding sandstone had washed down and consolidated over one side of, and into the dish, and it had to be carved out of its matrix like a fossil, with hammer, chisel and knife. When disengaged it was found to be cracked in several places. It contained a store of five big lumps of haematite, two quartz pebbles, one showing a bruised area; two shapeless portions of a large flint nodule, and about 12 freshwater bivalve shells of the living Nile Valley species, *Spatha cailliaudi* Martens. Whether these last were intended as palettes for the haematite-pounding outfit or represent the residue of a meal is unknown.

The form of the bowl is of individual type, the rim being slightly peaked at four opposing points, thereby emphasising the rectangularity of form.

Plate K. No. 3.—A globular pot with constricted top. Diameter of rim, 8 cm.; greatest circumference, 46.5 cm.; height, 17.5 cm.; diameter of flattened base, 4 cm. This pot is even more weathered than the others, due to its position on the fringe of sand-dunes in the immediate vicinity of site "V." It lay partly exposed in the shifting sand, and would appear to have been washed down the slope from the big site near by. Altitude, 160 feet above present lake. In constitution and technique the pot bears the same characters as the others. The surface is granulated by sand blast, and gives no clue to the original condition.

* The altitudes in the neighbourhood of mound "W" were obtained by means of a chain of dumpy-level readings, extending from the lake shore over five miles inland. Those for the Qasr el Sagha sites were fixed by the same method, only taking the temple at Qasr el Sagha as established datum (*i.e.*, 257 feet over present lake) and working down. The settlements were found to lie at approximately the same altitudes in both cases. The figures, however, will be checked by other methods next season.



FIG. 2.—SANDSTONE HILLOCK, SETTLEMENT SITE N.

Plate K. No. 4.—A small cup. Internal diameter at rim, 9.3 cm.; height, 6 cm.; thickness, about 0.8 cm.; diameter of flattened base, 5 cm. Found *in situ* about 6 inches beneath the surface at mound "W." One half is restored. It is made of the same badly fired clay as No. 1, and, like the others, is hand thrown.

Plate K. No. 5.—A saucer in rough red pottery with black core. Internal diameter, 12.3 cm.; height, 4 cm. Also found near the surface at mound "W."

In addition, this mound yielded no fewer than 11 cooking pots of similar manufacture to No. 1. Form differed but slightly, the most extreme variant from No. 1 type being here shown (Fig. 3).

(b) THE BONE IMPLEMENTS.—These form an interesting addition to our knowledge of this same Neolithic, or possibly Eneolithic, culture, the geographical distribution of which is at present so imperfectly known, but which, by means of the flint work alone, is traceable from the Siwa Oasis, through the Fayum Oasis, with southern outliers in the Kharga and Dakhla Oases, on eastwards to Wady el Arish, and up into Palestine (Gaza and Ascalon).

With one exception, these polished bone implements from the Fayum were surface finds, being collected from lacustrine clays in the vicinity of settlements. The first question as to their antiquity was reasonably settled later by the fortunate discovery of a broken bone point in a particularly well-sealed-in hearth, associated with a quantity of typical Fayum flints.

Fig. 3.—COOKING FOOD MOUND, W.

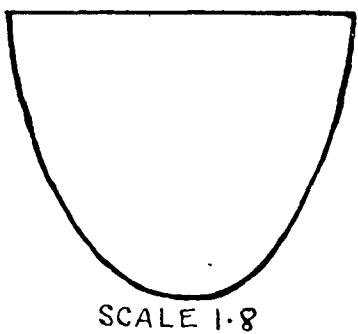
This hearth, at site "Z," lay 200 feet above present lake-level. The forms of the bone implements include harpoons, long and short, bevelled, or simply tapered points, and two other types—one an awl or borer; the other more complex, and probably a weapon of the chase. A selected few are here described.

1. *The Harpoons* (Plate K., Nos. 19, 21, 22).—No. 19 is a beautiful specimen of a delicate, unilateral harpoon, 8 cm. in length, with five well-cut straight and shallow barbs, set at the unusually low angle of 9°. In the Magdalenian harpoons the angle is more acute. The second barb was anciently broken off. The terminal barb is cut so near the tip that the mergence of the barb angle and the shaft forms the point of the implement. The barbs cover 5.8 cm. of the total length. The harpoon is a mahogany brown, smooth and polished, and shows a flattened section, with a shaft circumference of 2.5 cm. It is worked to a pointed basal extremity. No visible device for attachment is present, unlike the bone points, upon many of which encircling striæ have been scratched.

2. *The Bone Points*.—Nos. 8 and 10 are characteristic of the long, bevelled variety. No. 8 is a single bevelled point in perfect condition, 11 cm. in length, and some 2 cm. in circumference just above the bevel, thickening to a slight, though distinct, swelling of 2.5 cm. near the opposite end. The section is round. Above the swelling are several artificial striations around the shaft, providing a hold on the polished surface for methods of attachment. The bone has acquired the same rich brown patina as the harpoon, and they come from the same lacustrine basin.

No. 10 is of similar, single bevel type, 9.6 cm. in length, from near Qasr el Sagra. The beveling in this case, however, is rather deeply grooved—a fact, perhaps, due more to the splintering nature of the bone than to the intentions of the maker. The opposite end is striated in the same manner as in No. 8. The colour of the bone is unchanged, except for some small black stains.

Nos. 5 and 7 are of another type, unbevelled, and striated at both ends.



No. 13 is a short point, barely 5 cm. in length. This simple type is tapered off to pointed ends, and shows neither bevelling nor striations. Others, such as Nos. 12, 15 and 17, have one end pointed only.

No. 18 is a unique specimen, 8·5 cm. in length, slightly curved, and with a protuberance or spur 2·4 cm. from the tip. The butt end is squared off, and the stem is adorned with a grooved spiral, twisting round the shaft, terminating 1·4 cm. from the spur. An incised line emphasises this spiral grooving. An incomplete eyehole has been bored nearly half-way through the bone 1·2 cm. from the butt end, and the first stages of another intended eyehole are evident just above it. The implement was found near site "T."

No. 20 is an awl formed on a longitudinally split bone.

GERTRUDE CATON-TOMPSON.

Anthropology: Physical.

Buxton.

On the Reconstruction of Cranial Capacity from External Measurements. By L. H. Dudley Buxton, M.A. **97**

During the course of certain researches on prehistoric man one of my colleagues was anxious to know how far the Lee-Pearson reconstruction formulæ* for cranial capacity were applicable to specialised races. I, therefore, undertook to test the formulæ. I selected "generalised formula No. 10":—capacity = $\cdot 000365$ (length \times breadth \times auricular height) + 359·34, which is clearly the best for male crania and on which the authors lay most emphasis. They apparently do not think that the formula should be applied to specialised races, as they expressly exclude the negro, although they state that it would be difficult to imagine "three more widely "diverse races [than the Ainu, Germans and Naqada]." I selected as my test case the Eskimo—a race that appears to possess very individual characters. Using the data in Fürst and Hansen's "Crania Groenlandica," for 160 ♂ crania I found a correlation of $\cdot 8319 \pm \cdot 0164$ between capacity and (length \times breadth \times auricular height). The reconstruction formula is capacity = $\cdot 00049$ (length \times breadth \times auricular height) — 37·2. Clearly we have here a race very "diverse" from those selected by Lee and Pearson, and the application of their formula to Eskimo skulls must give—and does give in all the cases I have tried—very unsatisfactory results. This, I think, the devisers of the original formula would be the first to admit. Such results show that we must be very careful in applying the formula to prehistoric skulls. Some clearly come within the limits of the original formula; others are, it seems not unlikely, as different as the Eskimo. We need, then, a method of testing whether the Lee-Pearson formula is likely to give good results. In some cases the answer will be supplied by a study of racial affinities. In others the matter is more difficult. I suggest tentatively that, wherever we have a fairly long series of skulls of the type we are studying, we should work out a regression formula for that series. If it does not differ significantly from the Lee-Pearson formula, the latter is certainly the best that we may expect to get according to the laws of chance. If the difference is significant—but the probable error of our new formula is, owing to the shortness of the series, too big to allow us to be certain of getting even approximately correct results—where we cannot measure the capacity we cannot calculate it; to calculate it from a formula which is not intended for the specialised race we are studying will not give satisfactory results. I am aware of the crudity of my suggested test, and should be grateful if the authors of the original formula could give us some test of applicability, especially in regard to prehistoric skulls, which are so often in a fragmentary condition.

L. H. D. BUXTON.

* *Phil. Trans.*, 196A. 1901. 247.

Anthropology, Physical.

Sollas.

The Chancelade Skull. By Professor W. J. Sollas, Sc.D., F.R.S.

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That the ancestors of some at least of the Magdalenian peoples were to be found among the existing races of the Arctic regions, particularly those displaying some Mongoloid characters, was very early asserted by distinguished French archæologists, whose views received later strong support from Prof. Dupont (1872) and Sir W. Boyd Dawkins (1874), who both concluded from a large body of evidence that the Eskimos are the surviving representatives of the reindeer hunters.

A welcome and unexpected confirmation of this conclusion was afforded in 1889 by Professor Testut,* who, as a result of a masterly investigation of the famous Chancelade skull, arrived at the conclusion that the skulls which it most closely resembles are those of the Eskimos.

This determination has been confirmed and accepted by all the leading Continental anthropologists who have since examined the evidence. I may mention in particular the late Giuffrida-Ruggeri, a brilliant investigator, who has brought into strong relief the characters which distinguish the Chancelade from the Crô-Magnon skulls, and my friend Professor Boule, who has lately done much to restore the skull to its original state, and to whose kindness I am indebted for an excellent cast of it.

Of late years, however, doubts have been cast on Testut's identification. The first indication of a difference of opinion made its appearance in an unsigned review in *Nature*, where we read: "Those who have studied the Chancelade skeleton in the Museum of Périgueux will hesitate to accept its identification by Professor Sollas [sic] as Eskimo in character"†—a statement which is both misleading and historically inexact.

These doubts have recently culminated in an emphatic denial given by Sir Arthur Keith, who asserts that, so far from being an Eskimo, the Chancelade man was "a member of a racial stock of a true European kind."‡

As the question is of great importance§ and this is the first time that the expression of an unfavourable opinion of Professor Testut's conclusion has been accompanied by arguments and data, I have been led to undertake an independent examination of the Chancelade skull, and after renewing my acquaintance with the actual fossil at Périgueux, I have studied in greater detail the cast provided me by Professor Boule.

As I propose to give later a full and systematic account of the results of my investigation, I content myself for the present with a brief—and I hope impartial—examination of the grounds on which Professor Testut's conclusion is disputed. The authoritative statements of so distinguished an anatomist as my friend Sir Arthur Keith would seem to demand no less than this. As a preliminary, I should like to point out that in some matters we are in complete agreement; thus, when Sir A. Keith insists on the fact that the Chancelade skull is of a modern and not a primitive type, I readily admit it, but when we are left to infer that this

* L. Testut, "Recherches anthropologiques sur le Squelette quaternaire de Chancelade," *Bull. Soc. d'Anthr. de Lyon*, t. vii, 1889.

† *Nature*, Vol. LXXXVII, p. 405, 1912.

‡ Sir A. Keith, "The Antiquity of Man," 2nd Ed., Vol. I, pp. 80-87. London, 1925. Also the following:—"I have examined this famous specimen and am familiar with the cranial features of the Eskimo, and the conclusion I have been obliged to form is that the Chancelade skull, while possessing a few superficial resemblances to Eskimo skulls, is in its essential character just as European as the people of England and France to-day."—MAN, Oct. 1924, No. 115, p. 157.

§ "Chancelade skull has to shoulder, almost alone, the heavy responsibility of representing to us the men of Europe during that closing phase of the Ice Age which is marked by the Magdalenian culture."

affords a support to his argument, I must demur, for the same is equally true of the Eskimo skull. I am aware that certain anatomists have endeavoured, under the influence of prepossessions, to assign a primitive character to the Eskimo skull, but they do not seem to have succeeded. The question has been searchingly investigated by Fürst and Hansen, who conclude with the statement that "The Greenland Eskimo "cranium is not a cranium of a primitive race."*

Again, when Sir A. Keith, speaking of the nasal spine of Chancelade man, remarks that it presents us with nothing primitive or simian, but with "a feature " which the Chancelade man shares with most modern Europeans," I can only agree; but again with the addition that the same may be said of the nasal spine of the Eskimos. So, too, with the description of the Chancelade skull viewed in "norma lateralis."

Entering now into details, we may begin with the cranial capacity, for in this important character the skull is exceptional: it is no less than 1710 c.c., as we are assured by Testut,† who measured it directly by gauging it with mustard seed, and controlled his results by a study of the effect upon his measurements which might be produced by the substitution of mustard seed for shot. He remarks that he considers his direct determination (1730 c.c.) to be correct within a possible error of ± 5 to 10 c.c., and then deducts 20 c.c. to compensate for the absence of the body of the sphenoid.‡

Sir A. Keith, who makes no reference to Testut's result, gives the capacity as 1530 c.c., a number which he obtains by applying "the Lee-Pearson formula for " estimating the size of the brain " from the linear measurements of length, breadth and height.

Thus a serious disagreement presents itself at the outset and it is of considerable importance, since it affects not only the trustworthiness of Testut as an observer, but also the significance of some of the features we may have to consider later. We must, therefore, investigate it in some detail.

The particular formulae selected by Sir A. Keith from those given by Lee and Pearson is not indicated, but presumably it is that which these authors distinguish as (9).§ Neither is the method of applying it given, but on p. 596 of "The Antiquity " of Man," to which we are referred, we find an example which occurs in a discussion of the Piltdown skull. The formula there employed is:—

$$0.4 (190 \times 140 \times 112) + 206 = 1397 \text{ c.c.}$$

On working this out, however, we obtain an impossible result. At first sight it would appear that 0.4 should have been 0.0004, but on referring to Lee and Pearson, I can find no formula which agrees with that adopted by Sir A. Keith. Their formula for German males is:—

$$0.000332 (L \times B \times H) + 415.34 \left(\pm \frac{55.41}{\sqrt{n}} \right)$$

The factors 0.0004 and 206 do, however, occur in their formula (14) ♀, which is given "to find the capacity from measurements on the living head," and it runs as follows:—

$$C = 0.0004 (l - 11) (b - 11) (h \times 11) + 206.6$$

Thus Sir A. Keith's formula is a hybrid produced by crossing Lee and Pearson formula No. (9) with their formula No. (14), i.e., for a man's skull and a woman's head.

* Carl M. Fürst and Fr. C. C. Hansen, "Crania grøenlandica," p. 225. Copenhagen, 1915.

† Testut, *op. cit.*, pp. 31, 32.

‡ Testut, *op. cit.*, p. 83.

§ Lee and Pearson, "V. Data for the Problem of Evolution in Man." *Phil. Trans.*, Vol. CXCVI, A, p. 252, 1901 (not 1899 as given by Sir A. Keith, *op. cit.*, p. 596).

Making use of Lee and Pearson's formula (9) I obtain for Chancelade :—

$$0.000332 (193 \times 139 \times 124) + 415.32 \pm 55.41 = 1522.32 \pm 55.41,$$

and consequently a possible total of 1577.73.

But this is not all; to estimate the possible error to which their formulæ might give rise, Lee and Pearson selected 20 skulls at random and then compared their known capacity with that obtained by calculation. Only one of these skulls had a known capacity exceeding 1700 c.c. (1705 c.c.) and the application of formula (9) to this gave an error of no less than — 149 c.c.

Further, by applying the formula to some Eskimo skulls of unusually high capacity I found even greater discrepancies than this; thus for skull No. 146* we have :—

$$0.000332 (191 \times 140 \times 128) + 415.34 = 1552.34$$

or 1608 c.c. if we add the possible error of 55.41. But the actual capacity is 1755 c.c.

And again for skull No. 237 :—

$$0.000332 (186 \times 139 \times 130) + 415.34 = 1531.34$$

or, with the possible error of 55.41, 1587 c.c.; the actual capacity being 1740 c.c., so that instead of an error of 55.41 we actually have one of 209 c.c., and if we apply this correction to the result obtained for the Chancelade skull we have $1522 + 209 = 1731$ c.c., which corresponds with Testut's result before he made an estimated deduction of 20 c.c.

I think, therefore, that the *a priori* probability in favour of Testut's measurement is not diminished by a misplaced application of Lee and Pearson's formula.

Passing over some generally descriptive matter we now come to the nose, a feature which has been regarded by all anatomists who have especially studied the Eskimo skull as affording a distinctive racial character: it is remarkably narrow in proportion to its height. The relation of breadth to height is represented by an index: $B \times 100/H = \text{index}$. In the Chancelade skull the index is 42.6: in the Greenland Eskimo, the average for the race is 42.99, a remarkably close correspondence.

Sir A. Keith states that the Chancelade nose was long and straight, but adds that "unfortunately a part of the bridge was broken away, so we cannot tell its degree of prominence . . ."† But Testut‡ tells us that the nasal bones were complete when the skull was found, but were broken in making a cast, so that all he can now say is that the nasal bones were very narrow [an Eskimo character] and strongly bent to one side—the left, and that on proceeding from the nasion they were inflected so as to describe a curve directed forwards and upwards until they approached the horizontal. This is a description which would apply very accurately to the nose of most Eskimos.

In speaking of the face Sir A. Keith cannot avoid the admission that it is very wide and distinguished by the outward bend of the zygomatic arches "as they turn inwards on the face at the cheeks," and he adds: "This is a feature which is particularly developed in the skulls of the Eskimo," but then continues ". . . and it is chiefly upon the possession of this feature that the Chancelade man has had the misfortune to be assigned to a Mongoloid race." This surprising statement § is in direct contradiction to the facts, and, further, the question is not whether the Chancelade skull is Mongoloid, but whether it is Eskimo or not.

* Fürst and Hansen, *op. cit.*, p. XIV.

† Sir A. Keith, *op. cit.*, p. 85.

‡ L. Testut, *op. cit.*, pp. 36-37.

§ I presume from the context that the Mongoloid race referred to is the Eskimo.

Testut, whose work is a model of research, lays no particular stress on this character of the face, but simply enumerates it along with several others as one of the features which ally the skull with those of the Eskimo. Thus, towards the close of his memoir* he gives the following "tableau synoptique":—

	♂ Chancelade. Esquimaux. Crô-Magnon		
Indice crânien	72.02	72.19	73.76
Capacité crânienne	1710	1520	1590
Indice palatin	67.93	68.4	71.1
Indice nasal	42.50	42.62	45.9
Indice orbitaire	86.97	87.8	61.36
Angle naso-malaire de Flower	145.	144.	?
Profondeur des orbites	56.5	57.7	?
Diamètre transverse de la face	140.	135.	143.
Ind. de haut de la face (dia. trans. = 100)	72.85	72.2	63.41
Indice de larg. de la face (lig. naso-alv. = 100)	131	134	144
	144	150	160
	154	157	?
Torsion de l'humérus	140	143	?
Taille	1.50	1.54	1.86

It was on this remarkable assemblage of similar characters that Testut based his conclusions, or, to give his own words: "Ces rapprochements de chiffres légitimeront nos conclusions en faveur d'une parenté plus ou moins étroit existant entre l'homme de Chancelade et les Esquimaux actuels." It is the same with Ruggeri who, when he sums up very briefly the most salient resemblances of the Chancelade skull to the Eskimo does not even mention the breadth of the face. The features which do impress him are the low stature (about 1.50 m.) accompanied by a voluminous skull (cranial capacity, 1700 c.c.), marked hypsistenocephaly, the vault having a typical roof-like form, the face strongly leptorrhine and the orbits with a massive lower margin.

We now come to the orbits of which Sir A. Keith writes: "The eye-sockets are of moderate dimensions—not deep from upper to lower margin as in Eskimo skulls."† Statements such as this, so directly contrary to the fact, enable us to understand how this author has been led to an erroneous conclusion. Testut gives measurements of the height and breadth of the orbits, which I am able to confirm from my own observations. They are as follows:—

	Breadth.	Height.
Left orbit	39 or 38 mm.	32 mm.
Right orbit	37 mm.	34 mm.

He remarks on the inequality of the two orbits and finds it difficult to decide whether it is original or produced in the restoration of the face, but inclines in favour of the former alternative.

From the average of the two he obtains the mean index (height \times 100/B), which is 86.97. In the Greenland Eskimo the mean index is 85.04.‡ Thus, the Chancelade orbits are slightly more hypsiconch than the average Eskimos, i.e., not less deep from upper to lower margin, but slightly deeper.

Finally we have to consider those characters of the lower jaw which Sir A. Keith considers inconsistent with Eskimo affinities. "He had not the projecting jowls of the Eskimo, the distance between the angles of his lower jaw was only

* L. Testut, *op. cit.*, p. 111.

† Sir A. Keith, *op. cit.*, p. 86.

‡ As given by Fürst and Hansen and based on the measurements of 310 skulls.

" 93 mm., whereas in the Eskimo and other races to be mentioned later, this—the bигonial diameter—will often amount to 110 mm., or even 120 mm."

The mean length of the biangular diameter determined by Fürst and Hansen from 100 mandibles of the Greenland Eskimos is 109.7 ± 0.65 , but the range is given as from 90 to 134 m.m. The minimum, however, is only found in the female mandible, but 94 mm. has been recorded from one of undetermined sex, and 95 mm. from a man's mandible.

It is more important, however, to observe that the Chancelade mandible is abnormal and that the exceptionally low value of the biangular diameter is capable of explanation. In the course of his life the Chancelade man received a terrible blow on the right side of his head, which fractured the skull in the temporal region and drove the broken fragment of bone inwards. The line of fracture runs through the lower part of the parietal and the upper part of the squamosal bone and bounds an oval area 63 mm. in breadth by 50 mm. in height. Testut cites the case of a similar fracture experienced by a coachman whose horses ran away with him. The coachman succumbed to his injury; the man of Chancelade, without medical assistance, but favoured by his splendid vitality, survived. The wound healed completely; a cicatrice remains to record the injury. Testut also calls attention to certain differences which distinguish the right from the left side of the mandibular ramus and proposes to explain them as a consequence of the accident which may be supposed with great probability to have injured the right temporal and masseter muscles, so that the work of mastication was imposed chiefly or entirely on the corresponding muscles of the left side.

If now we examine the left ramus, we find nothing very remarkable about it beyond the facts pointed out by Testut. It is in every way comparable with the ramus of an Eskimo jaw; and in particular it slants outwards as it descends, so that on the inner side a vertical line drawn from the lingula downwards meets a horizontal surface 6 mm. inwards of the inner boundary of the lower edge of the ramus. A similar line drawn upwards from the region of the gonion on reaching the level of the edge of the sigmoidal notch is removed from it by the same distance of 6 mm.

In the right ramus, however, this outward slope has disappeared or is even slightly reversed, so that the gonion lies some 4 or 5 mm. inwards from the foot of a vertical line dropped from the middle of the sigmoid notch, *i.e.*, 8 or 9 mm. nearer the medial sagittal line than it should do if it were truly normal. It is, therefore, extremely probable that before the accident the jaw might have presented a biangular breadth of over 100 mm., as the jaws of nearly 20 per cent. of the existing Greenland Eskimos are known to do. "The great development of the chin," to which Sir A. Keith refers, is by no means uncommon among the Eskimos; it may be added that the Chancelade chin is not only well developed but pointed, and even pointed chins are not unknown among the Eskimos.

Of the other characters presented by the lower jaw and not alluded to by Sir A. Keith, I need only mention the breadth of the ramus, for the existing Eskimos are distinguished by its magnitude. "It is in the breadth that we find the enormous dimensions, to which the lower jaws of other races can scarcely show any counterpart," so write Fürst and Hansen,* who give 39.95 ± 0.21 mm. as its mean value, a range of from 32 to 46 mm., and a central group of from 38–42 mm.

The breadth of the ramus in the Chancelade jaw is 43 mm.

This brings the critical part of my study to an end; it may serve to clear the way for the more complete systematic statement which I hope soon to have ready for publication.

W. J. SOLLAS.

* Fürst and Hansen, *op. cit.*, p. 169.

Anthropology Physical.

Pycraft.

Diagnoses of four Species and one Sub-species of the Genus *Homo*. By W. P. Pycraft. **99**

In a paper read at the Southampton meeting of the British Association I contended that a natural classification of man was impossible, unless we adopted the methods of the zoologists, and prepared the way for such a classification by recognising the existence of species of the genus *Homo*, as we recognise species of the genus *Felis*, or the genus *Fringilla*.

In the case of man, however, the task is complicated by the fact that, though it is easy enough to recognise several distinct types of man—"Yellow-races," Negroids, the "Nordic" and "Alpine," and so on—one cannot find "communities" of individuals displaying the same uniformity of physical characters that one finds in the case of all other Mammals. And this because man has never been subjected to the same factors of isolation, but has freely interbred with alien races.

Though this intermingling has confused our records, it has not destroyed them. It is still possible to find what we may call "specific types," and to detect the salient features of such types in various combinations in individuals of mixed descent.

The old conception of a "species" was based on the assumption that true species would never interbreed, or that the resultant hybrids would prove infertile. The fallacy of this view is proved up to the hilt by the facility with which sharply defined species, such as the mallard and the pintail, not to mention numerous others among the Anatidæ, and the various species of Phasianidæ, can be induced to interbreed, producing fertile hybrids. The Mammals furnish a number of similar cases.

What is to be understood by the term "species" was admirably expressed by Mr. Tate Regan, in his Presidential address at the Southampton meeting of the British Association. "A species," he says, "is a community, or a number of related communities, whose distinctive morphological characters are, in the opinion of a competent systematist, sufficiently definite to entitle it, or them, to a specific name. Groups of higher or lower ranks than species can be defined in a similar way. Thus, a sub-species is a community, or a number of related communities, whose distinctive morphological characters are not, in the systematist's opinion, sufficiently definite to merit a specific name, but are sufficient to demand a sub-specific name. Similarly a genus is a species, or a number of related species, whose distinctive morphological characters entitle it, or them, to generic rank."

"There are, of course, many species so distinct from all others and so uniform throughout their range that everyone is agreed about them; but frequently the limits and contents of a species, as of a genus, are a matter of opinion. No systematist has, or should have, any rule as to the amount of difference required for the recognition of a species, or a sub-species; he is guided by convenience. In practice it often happens that geographical forms, representing each other in different areas, are given only sub-specific rank, even when they are well defined, and that closely related forms, not easily distinguished, are given specific rank when they inhabit the same area but keep apart."

The soundness of this reasoning few will dispute. But in so far as man is concerned it may be urged that we do not find "communities" of species more or less sharply defined. This is true. Nor can we pick at random any individual from any given community, or any skull from a series representing any given community, and designate that man, or that skull, the type of the species represented by that community. Our specific characters, it would seem, can generally only be determined after intensive study of a large series: and they must be sought

for first among strongly differentiated types—Negro; Negrillo; Tasmanian; pre-Dravidian; Polynesian; "Mongoloid," and so on. The type selected, comparison with this will reveal the nature of the racial intermixture which has taken place in the rest of that species, as I have shown in the case of the Maoris and the Papuans, for example.

Where hybrids between two or more species of the Anatidæ, or the Phasianidæ, are concerned, we can detect the nature of the combination by means of the plumage, and also by structural differences, and this with little or no hesitation.

The same results of hybridisation are to be seen in the case of the genus *Homo*, and the detection of the nature of the cross will become increasingly easy as crania, and other morphological characters, become more intensively studied. The detection of this evidence constitutes a new field of anthropological research, of which the first-fruits were submitted in my paper just referred to, wherein will be found more detailed arguments.

This method, however far it may fall short of what we desire, will at least serve us infinitely better than the "average indices," which so far have formed the basis of our classification, and hence the incongruous results which they have yielded. Our classification, in short, if it is to express genetic affinity, and it is useless if it does not, must be based on morphological characters, and not on a compromise between physical characters, superficial and skeletal; and cultural characters borrowed from the ethnologist.

The following new species and sub-species are described in this Paper:—

ANDAMAN NEGRITO. *Homo mincophæus*.

Skull with glabella and supraciliary ridges moderately developed, frontal eminences paired; parietal bosses well developed, divided by a slight median ridge; orbits quadrangular, with the long axis oblique; nasals long, moderately wide; pyriform aperture sharply defined; nasal spine, long; mastoid small.

Type skull: Coll. Mus. Brit. (Nat. Hist.) Reg. No. 84.9.5.1. Andaman Islands:—L. 171. B. 139. (I. 81.) Bb. 136. (I. 79.) Bp. 92. Bn. 92. (I. 100-F. a. 81°.) Nw. 21. Nh. 48. (I. 31.) Np. 64. Bz. w. 124. (I. 52.) Oh. 31. Ow. 39. Brachycephalic. Hypsistenocephalic. Mesognathous. Leptorrhine. Leptoprosopic.

SUMATRAN NEGRITO. *Homo mincophæus sumatreensis*.

Skull with glabella and supraciliary ridges obsolete; frontal eminences slight; parietal eminences moderate; orbits quadrangular; nasals long and narrow.

Type skull: Coll. Mus. Brit. (Nat. Hist.) 84.4.23.1. "Kubu," Sumatra:—L. 173. B. 133. (I. 76.) Bb. 131. (I. 75.) Bp. 96. Bn. 99. (I. 96. F.a. 77.) Nw. 23. Nh. 45. (I. 50.) N.P. 62; Bz. 121 (I. 51). Mesaticephalic. Metrioccephalic. Orthognathous. Mesorrhine. Leptoprosopic. Fig. 6. Pl. IV. "Rep. Human Crania, Coll. Brit. Orn. Exped. Dutch New Guinea. 1916."

VEDDAH. *Homo ceylonensis*.

Skull with glabella feebly developed; supraciliary ridges well marked; frontal eminences conspicuous; parietal eminences well-marked; orbits small, quadrangular; nasals long, moderately wide; mesodont.

Type skull: Coll. Univ. Mus. Oxon. No. 736. Greenwell Coll.:—L. 186. B. 131. I. 70. Bb. 135. (I. 72.) Bp. 96. Bn. 99. (I. 97 F.a. 77.) Nw. 23. Nh. 49. (I. 47) Np. 66. Bz. 126. (I. 52.) Oh. 30. Ow. 37. Dolichocephalic. Metrioccephalic. Orthognathous. Leptorrhine. Leptoprosopic.

FUEGIAN. *Homo patachonicus*.

Skull: supra-orbital ridges heavy; external angular orbital processes protuberant: a slight median frontal ridge; temporal bullate; palate large, with a conspicuous "torus palatinus" megadont; orbits large.

Type skull: Coll. Mus. Brit. (Nat. Hist.) Reg. No. 99.4.27.1. Tierra del Fuego:—L. 193. B. 150 (I. 77) Bb. 147. (I. 76.) Bp. 108. Bn. 108. (I. 100 F.a. 80°) Nw. 23. Nh. 53. (I. 43) Np. 75. Bz. 142. (I. 52.) Oh. 33. Ow. 40. Mesocephalic. Metrioccephalic. Mesognathous. Leptorrhine. Leptoprosopic. Phænozygous.

The following, described in my Boskop Report, *J.R.A.I.*, LV., 1925, p. 179, were figured in my Report on human crania collected by the "British Ornithologists"

Union Expedition to Dutch New Guinea," 1916, Vol. 3 (London : Francis Edwards), Australian, Pl. II., Fig. 3. Polynesian, Pl. V., Fig. 5. The skulls of the Negro and Negrillo were not figured in the Boskop Report, but contours thereof, and of the species described here, will be given in the November number of MAN.

As I now wish to restrict the name *Homo sapiens* to the Nordic, the following changes in nomenclature have become necessary :—

AUSTRALIAN. *Homo antiquus.*

Homo sapiens antiquus, Pycraft, J.R.A.I., LV., 1925, p. 196.

POLYNESIAN. *Homo sandvicensis.*

Homo sapiens sandvicensis, Pycraft, J.R.A.I., LV., 1925, p. 196.

The Crô-Magnard was described but not specifically named in the Boskop Report. The name and description is as follows :—

CRÔ-MAGNARD. *Homo lartetii.*

Type skull : "The Old Man of Crô-Magnon." Crô-Magnon :—L. 205. B. 151. (I. 74.) Bb. 137. (I. 66) Bp. 118. Bn. 114. (I. 103. Fa. 79.) N. w. N. h. 52. Np. 70. Bz. 133. (I. 52.) Oh. 27. Ow. 42. Dolichocephalic. Tapeinocephalic. Mesognathous. Leptoprosopic.

In the comparison between this and the Boskop skull in that Report, delete the last three lines on p. 189, and the first two lines on p. 190 as far as "The auricular height." This escaped me in correcting the proofs of that Report.

W. P. PYCRAFT.

REVIEWS.

Ethnology.

Gregory.

The Menace of Colour. By Professor J. W. Gregory, D.Sc., F.R.S. 100 London : Seeley Service. 1925. pp. 255. 12s. 6d. net.

This work does not profess to be exhaustive. Indeed Asia, the continent which the author pronounces to be "to-day, as throughout historic times . . . " the greatest menace to Europe . . . the only continent which is a direct "and early danger to the European race" (p. 145) is dismissed in four pages. But, while not exhaustive, the volume is sufficiently comprehensive to justify its title and the subject is handled with a mastery which demands the attention of all who are interested in questions which must more and more engage the serious thought of statesmen and thinkers all the world over.

The largest sections of the book are those devoted to regions in which the author has had the opportunity of making observations and investigations on the spot. Colour problems in North America take up 90 pages, those of Africa upwards of 20 pages, and those of Australia, where the author was Professor of Geology in Melbourne University for several years, about 75 pages, exclusive of two intercalated chapters, one on the question "Can the White Man colonise the Tropics ?" the other on "Miscegenation and the Result of Racial Interbreeding," as well as of the paragraphs devoted to Australia in the final chapter giving a summary of the author's conclusions.

Of the various solutions proposed for the Negro problem in the United States that which appeals to him as the most promising of success is that of Maurice S. Evans, segregation within the United States through the establishment of "small self-contained agricultural negro communities . . . in friendly business association with the neighbouring whites" (p. 99); and it may be added that the data which he furnishes on pp. 72 and 74 as to the relative efficiency of negro and white labour, data which will probably be surprising to most people, though not to those who have given attention to such questions, seem to justify the hope that the negroes in America are capable of acquiring the requisite ability as small farmers.

It is impossible without taking up too much space even to touch on the author's treatment of the complicated South African problems, but it would be ignoring what is regarded by some as the main purport of the book to leave unnoticed the author's attitude to race problems in Australia. He has long been known as a keen advocate of the policy of a White Australia adopted by the Commonwealth immediately after its proclamation. He bases his advocacy on no arrogant conviction of the innate superiority of the European race. His plea is a rather curious one, that, considering the contributions of that race to humanity, and accepting the probability that the coloured races are to hold all Asia and Africa and to be predominant in South America, we should recognise it as only fair that the three smallest continents, Europe, North America and Australia, should be assigned to the white race (pp. 159, 242). It is on this ground, coupled with the belief that the association in mass of people of different race leads to jealousy, strife and race hatred (p. 241), that he regards the policy of a White Australia as desirable, but he admits (p. 157) that that policy must meet two serious objections: first, the difficulties raised by the exclusion of British subjects from a British territory and, second, the possible delay in the settlement of Australia. The first objection is met in a manner that is not likely to be accepted by the Indians who are most forward in making it. The refusal to allow an Indian to set up business in Australia, is, he argues, no more inconsistent with his status as a member of the empire, than the refusal of Glasgow to allow James Watt to start business in that city was inconsistent with his status as a Scotsman. As to the second objection, many will agree with him in holding this to be a minor consideration. There are many who consider that greater evils are likely to result from too rapid than from retarded development. That the White Australia policy is not merely desirable but physiologically and economically possible the author holds to be amply proved.

GEO. G. CHISHOLM.

Ethnology.

Stoddard.

Racial Realities in Europe. By Lothrop Stoddard, A.M., Ph.D. London. 101
C. Scribner's Sons. Pp. 252. $8\frac{1}{2} \times 5\frac{3}{4}$. 3 folding maps. 12s. 6d.

This is a vivacious running commentary on current political problems of Europe by an author who is sometimes quite well informed and who is well known for his passionate belief in the absolute superiority of the tall fair long-headed group known as the Nordic Race. One has difficulty in accepting his statement that England is $\frac{4}{5}$ Nordic and that its civilization is fundamentally Nordic; it is clear enough to those who have worked at physical anthropology that race-types are largely abstractions and that individuals approach them, but only occasionally illustrate them with any completeness. We cannot put men into Nordic, Alpine and Mediterranean classes as simply as Dr. Stoddard wishes, though he does allow that Germany has a large Nordic-Alpine element. The author is probably fairly correct in seeing the French people as at present mainly Alpine; his political speculations on this basis, however, go rather far. Perhaps the thought which is most necessary as an accompaniment to a book of this kind is that the values of various types in practical life can hardly as yet be estimated seriously and that in any case they are values relative to particular environments and particular circumstances. And there is the further thought that different types and aptitudes supplement one another and contribute to the enrichment of civilization. One could imagine another author who was impressed with the value of, say, Switzerland to European life writing a very different book of at least as much value. Dr. Stoddard's work is readable, stimulating and should be taken with the moderation and with the reserve which is appropriate in using all stimulants.

H. J. F.

PROCEEDINGS OF SOCIETIES.

Anthropology.

British Association.

The Proceedings of Section H (Anthropology) at the twenty-fifth Annual Meeting of the British Association for the Advancement of Science, Southampton, 102 August 26 to September 2, 1925.

Section H met under the Presidency of DR. THOMAS H. ASHBY, Director of the British School of Archaeology at Rome, who took as the subject of his Presidential Address, "Engineering in Ancient Rome," dealing with the road, water and drainage systems, etc., of the city and its environs, describing the existing remains and demonstrating the methods by which the Romans had solved the engineering problems which they met in the administration of a great city and the organisation of their Empire.

In view of DR. ASHBY's close connection with Roman studies in this country, arrangements had been made for a number of papers dealing with Roman Britain to be presented to the Section. These filled a whole day of the programme, and covered the whole of the more important investigations now being carried on, with one or two exceptions. Among these were accounts of recent excavations in Scotland of MR. S. N. MILLER, and in Wales by DR. R. E. MORTIMER WHEELER; descriptions of the very interesting Roman Potter's Hut in the New Forest by MR. HEYWOOD SUMNER; of Roman Camps at Cawthorn, near Pickering, Yorks. (these appear to be a new kind of Roman fortification) by MR. IAN A. RICHMOND, and of recent excavations at Wroxeter by MR. DONALD ATKINSON. MR. ST. CLAIR BADDELEY's account of the Chedworth Roman villa and its groups was a study of a great Roman villa in relation to its environment, social, administrative and industrial, as a single organism, on lines of "aggregation" which, it was suggested, might prove fruitful on other sites. Archaeological studies outside Britain included papers on the recently discovered "Precinct of Tanit," a Punic burial ground near the so-called Ports of Punic Carthage, which is supposed to have been connected with child sacrifice. SIR FLINDERS PETRIE's "Earliest Civilisation of Egypt" dealt in fuller detail with the results of recent excavations described in the last issue of MAN. MR. S. CARSON described further excavations on a site in Macedonia which has produced relics of the early Aegean culture and of the Bronze Age and presented a paper, on behalf of MR. W. A. HEURTLEY, on excavations on two sites in the Vardar Valley. These showed three strata belonging to the Middle Helladic, Bronze and Iron Ages respectively, the last phase of the Bronze Age having been destroyed by fire. The general conclusion was that a large homogeneous body of invaders was at the borders of the Greek world at the beginning of the eleventh century B.C.

Turning to communications which dealt with the earlier phases of prehistory, first place must be given to the account of the discovery of the Galilean skull, by MR. TURVILLE-PETRE, and the description of the skull by SIR ARTHUR KEITH, which was certainly the most important item in the programme of the Section and perhaps it would be no exaggeration to say of the whole meeting of the Association. The circumstances of the discovery of this relic of early man in a cave near the Sea of Galilee are by this time well known. MR. TURVILLE-PETRE described the stratification of the deposits in the cave, which contain remains datable from recent times to the Bronze Age. Under these was a layer of fallen rock, beneath which the frontal bone of the skull was discovered in association with implements which were stated by MISS GARROD in the discussion which followed the paper to be unquestionably Mousterian. Careful examination failed to reveal any possibility of the skull being a later introduction by burial or otherwise into the deposits in which it was found. SIR ARTHUR KEITH demonstrated the various anatomical details in which

the fragment of the skull conformed to the Neanderthal type, but pointed to certain particulars in which it differed, notably in the greater height of the vault, and in the thinness of the bone, a characteristic, however, which might possibly be due to its being female. These communications dealing with the skull and its discovery will be presented at a meeting of the Royal Anthropological Institute in the course of the current month.

The question of the antiquity of man was raised by SIR W. BOYD DAWKINS in a communication in which he deprecated dating by terms of years " and upheld " strongly the view that man disappeared from Europe during the submergence " of the Atlantic border, a period separating the Prehistoric from the Pleistocene " Period." MR. L. S. PALMER and MR. A. C. HINTON described the fauna of some Pleistocene gravels at Clevedon, Somerset, formed about late Mousterian or Aurignacian times. MISS GARROD dealt with the Upper Palæolithic Age in Britain, arguing from certain divergencies in the various industries that England was to a certain extent " provincial " and cut off from her neighbours to the South in Magdalenian times, and MR. O. G. S. CRAWFORD discussed the effect of climate on migration in the Neolithic Age in Britain. MR. T. F. HEWER described the excavation of some round barrows in the Mendips which have produced beakers and cinerary urns and are peculiarly rich in flint implements. PROFESSOR H. J. FLEURE presented on behalf of M. LE ROUZIC, who was unfortunately unable to be present, a discussion of the problems of the Megaliths of Le Morbihan, Brittany, in which he assigned the monuments to the early Age of Metal. Some of the small mounds marked by a menhir are pre-metal; monuments built of large blocks are later in date. This civilisation lasted down to the Iron Age and probably to the coming of the Romans. COLONEL DE GUÉRIN described the megalithic culture of Guernsey. MR. E. TOULMIN NICOLLE gave an account of the excavation of the important barrow of La Hogue Bie, Jersey, which had brought to light a passage dolmen in a perfect state of preservation, cruciform in plan and oriented east and west. On one of the uprights were about 20 cup-marks. The floor had been rifled, but some human bones, and pottery of a ritual or votive character were discovered. The monument belongs to a transition period between the Neolithic and Bronze Ages.

MR. H. J. E. PEAKE exhibited and described a number of maps showing the distribution of archæological remains in which attention was called, in particular, to the value of the Ordnance Survey $\frac{1}{50000}$ map of Britain for this work.

In physical anthropology, MR. PYCRAFT dealt with the classification of man in a paper of which part is published in this issue of MAN. MR. R. N. SALAMAN dealt with the inheritance of facial type among Jews, and MR. TALBOT RICE gave the results of observations made among the modern inhabitants of the region around Hillah, who belong to the normal type of the North Arabic world, and were classified by him as " Semites "—members of the Mediterranean group. Among the ethnographical papers which filled the programme of one day, mention may be made of CAPTAIN HILTON-SIMPSON's description of the cinematograph films taken by MR. HAESELER among the Shawiya of the Aures, MR. HAMBLY's analysis of the distribution of tatu and its anthropological significance, MISS BLACKMAN's description of pottery-making methods in ancient and modern Egypt and CAPTAIN PITTRIVERS's account of the people of Aua, one of the Bismarck Archipelago group to the north-east of New Guinea. MR. E. J. WAYLAND exhibited an interesting series of stone tools from Uganda and MR. CARLINE a series of photographs illustrating the Bosnian house. A suggestive communication by PROFESSOR H. J. ROSE offered an explanation of Ritual Combat as an offering of courage or skill in arms analogous to a sacrifice of blood or flesh.

CORRESPONDENCE.

Egypt: Archæology.

Brunton.

Early Man in Egypt.*To the Editor of MAN.*

SIR,—With reference to Sir Flinders Petrie's article in MAN, 1925, 78, I **103** may be permitted to point out one or two facts which are of importance, but which have so far not been publicly referred to.

The early Predynastic Egyptians buried in the cemeteries at Badari (the "Badarian" Egyptians) were users of copper. In one quite untouched grave lay a child. Round the neck was a string of typical "Badarian" beads, which I removed myself. Two or three beads, made of narrow copper ribbon, were wound round the stone beads. In another grave, partly plundered, was a stout copper pin or borer.

The "Badarian" culture (to be carefully distinguished from the "Fayum" culture of Miss Caton-Thompson) has the strongest affinity with the Nubian—most especially in the pottery forms and technique. The "rippled" ware is common to the south of Egypt, though it is no doubt later in date. Dr. D. E. Derry, who has examined some 50 of the skulls from Badari, states that the type is essentially the same as the Predynastic Egyptian, but with some Negroid tendencies.

GUY BRUNTON.

Africa: Archæology.

Thomas.

Palæolithic Cave Paintings.*To the Editor of MAN.***104**

SIR,—In MAN, 1923, 131, I made the statement, that Obermaier's text to the plates of Hadschra Maktaba (Munich : Kurt Wolff) would connect the rock paintings of Africa Minor with the palæolithic art of southern France and Spain, and added that the two series appear to be formally identical and are probably contemporaneous. These inferences turn out to be erroneous now that the whole work is before us. Obermaier suspends judgment as to the age; he holds that a portion of the African products may be late palæolithic, but, bearing in mind that the stages of African art grade into one another down to the present day, he holds that the chronological difficulties raised by a theory of contemporaneity are very considerable. Not only so, but the almost complete absence of rock art from Mauritania and Numidia, compared with the more southerly districts, makes it clear that the art, even if contemporaneous, was independent in the two areas. This is further made clear by the history of the two schools, for that of Spain remained true to nature, while in Africa several stages are distinguishable, ending in a schematic and lifeless style.

Even in form the two series cannot be regarded as identical, for, though individual African scenes are as great art in their way as the best products of Spain, as a whole the performance of Africa Minor is considerably behind that of the Iberian peninsula; human portraiture is especially weak.

My apologies are due to Mr. Tristram for basing a criticism of his theory upon what turns out to be an unsound foundation. But Obermaier's view, based on the topography and orientation of the material now published, is that in Africa Minor hunting magic was at the root of the practice of this art. I have, at any rate, not cited against Mr. Tristram one who really supports his view.

NORTHCOTE W. THOMAS.



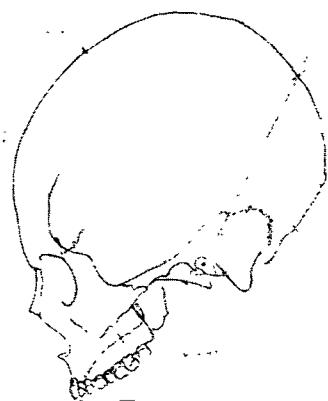


FIG. 1.—NEGRO.



FIG. 2.—NEGRILLO.

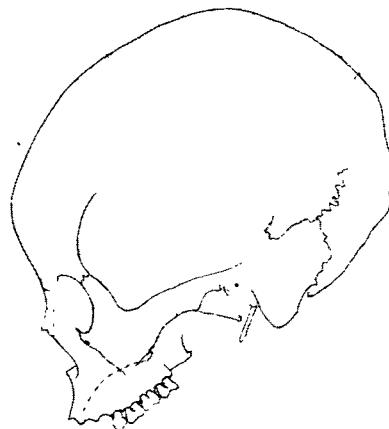


FIG. 3.—NEGRITO



FIG. 4.—VEDDAH.

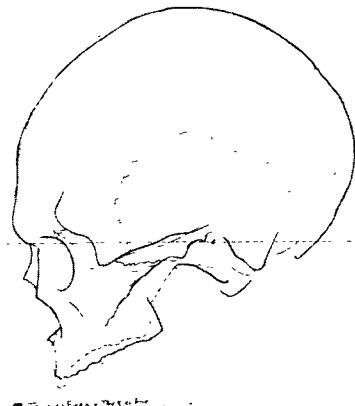
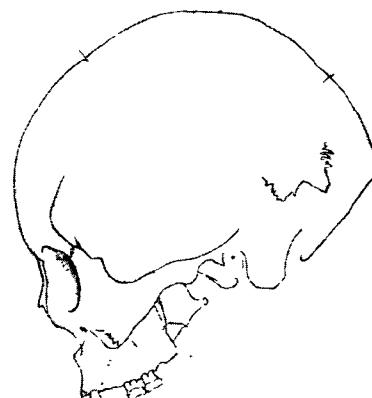
FIG. 5.—FUEGIAN THICK LINE.
TEHUELCHE THIN LINE.

FIG. 6.—I'NU BHOTIA.

TYPES OF POST-MOUSTERIAN MAN.

The dotted line above the alveolar border answers to the contour of the palate. Where this line is intersected the limit of the palatine is indicated. The intersecting of the contour indicated the bregma and lambda.

ORIGINAL ARTICLES.

With Plate L.

Anthropology: Physical.

Pycraft.

On the Recognition of several Species of Post-Mousterian Man: and the Need for superseding the Frankfort Base-Line. By W. P. 105
Pycraft. With Plate L.

At the Southampton meeting of the British Association I brought forward a plea for the recognition of specific types in the Genus *Homo*, and a drastic revision of our methods of craniology. Desiring, at the earliest possible moment, to place on record the diagnoses of four species, and one sub-species described at that meeting by way of a beginning, I published a brief summary of my contentions in *MAN*, 99, together with the diagnoses. The complete paper read at that meeting and the description of two additional species, now follow.

In preparing my Report on the Boskop skull I became more and more convinced of the absolute necessity of breaking away from the traditional conception of the Genus *Homo*, and definitely recognising the existence of an indefinite number of species, and sub-species. I say "definitely recognising" advisedly, because the proposal is by no means novel, since Giuffrida Ruggeri, Lapouge, Ripley, and others, have already done this in a loose way. So far, however, their work in this regard has been ignored by anthropologists. Indeed, this fate was inevitable, since their "species" were never properly defined.

It is a dreadful thing to say, and I hope I shall be forgiven for saying it—but our conception, as anthropologists, of "Races" and "Species" and "Somatological Units" is in a state of "mush." Compare the schemes for the Classification of Man which have been published by anthropologists, British, Continental, and American—and I have just added another myself—and see what you can make of them! There is not one—except, of course, mine, and I am prepared already to "pick holes" in that—which will stand the test of criticism.

Why is this? Briefly, because all have been built up of materials which cannot possibly hang together. Abstract conceptions, ethnical and superficial somatological characters, have been hopelessly mixed up. As a sample of such "classification," let me take that of Dr. Griffith Taylor, given in his paper, "The Evolution and Distribution of Race, Culture, and Language" (*Geographical Review*, XI, 1922, p. 54). He suggests a zonal distribution, based on the average cephalic index of various peoples; and ranges all the main races strictly in order according to their average cephalic index. It would be hard to put forward a more unconvincing proposal. Now, let us see where the pursuit of "cephalic indices" will lead us in this matter. Dr. Griffith Taylor's scheme is briefly this:—

ZONE I.—Negrito type: Negrillos; Negritos, all somewhat mixed; and Tasmanians, which are Negrito half-castes with the Dravidian-Australians.

ZONE II.—Lower Negro: Sudanese and West Coast races; Papuans and Southern Melanesians.

ZONE III.—Higher Negro, or Mousterian races: Bantu, Australians; Botoctudo; Dravidians.

ZONE IV.—Hamites and Semites; Ethiopians, Masai, Hottentots; Higher Melanesians; Iberians, Copts, Etruscans, Portuguese; Panjabi, Igorots; Moriots; Micronesians; Eskimo; Hurons: Seri; and Yahgans.

ZONE V.—Nordic races: The higher Aryans: the Alpines: Alpines in Asia; Palaeo-Siberians, Tungus, &c. Tibeto-Chinese; Late Aryan and Alpine peoples in America.

It would be difficult, surely, to produce a more incongruous mixture: or a better illustration of the hopelessness of obtaining any results, which can be of the slightest use, derived from the appeal to "average indices" of any kind whatsoever. "Culture and Language" have no place in Physical Anthropology. They belong to another "Universe of Discourse."

The futility of our present methods of divination in endeavouring to determine the status of any given race, is well shown in the efforts which have been made to define what is meant by a Polynesian, an Australian, Papuan, or Tasmanian, for example.

In 1915 I was writing a Report on a Collection of Skulls from New Guinea, and it became imperative to compare the Papuan with Polynesian skulls: since it was clear that these Papuan skulls were, many of them, largely infused with Polynesian blood. The only figure of a skull I could find, in any of the numerous books I consulted, which was definitely asserted to be Polynesian, most emphatically was *not* of that race, or as I now prefer to say, "Species." All these books gave me the colour of the skin, and eyes, the character and colour of the hair, stature, and so on, but absolutely no information about the skull. I therefore had to find my Polynesian. The result of that piece of investigation is given in the Report referred to.

The Polynesian skull, I found, in short, is one of the most easily distinguished of all human skulls; as I hope to show presently. Taking this as a standard of comparison it was possible to demonstrate Polynesian intermixture, not only among the Papuans, but also throughout the whole of the Pacific Islands.

The Maories, whose Polynesian blood none will doubt, can be shown to be by no means racially pure. The Moriori conform far more uniformly to my Polynesian standard: and this is important; inasmuch as Elliott Smith holds that the Moriori are of the same stock as the "Armenoids," who, he contends, originated in the Hindu Kush, and left witness of their wanderings in Egyptian tombs, and British graves! But be this as it may, the Polynesian type of skull has an existence in fact; and is not an abstract conception, as are so many of our standards to-day. But to this theme I must return.

The Tasmanian, again, by almost universal consent, is a hybrid race. But here the consensus of opinion ends: for the most diverse opinions have been expressed as to its admixture. So great an authority as Dr. Haddon tells us that the Tasmanian may be regarded as a somewhat generalized variety of the Negrito-Papuan stock . . . Even Dr. Duckworth refuses them recognition as a separate "race"; merging them in his "Group I," whose distribution is given as "Australia with Tasmania, Melanesia, New Guinea, sporadically throughout Oceania, and also in Africa."

This is, to say the least, a somewhat nebulous "Group": and the place of the Tasmanian is *not* within it. It would indeed be hard to find a skull-type more pronounced, and unmistakeable, than that of the Tasmanian. It has left an indelible mark in New Guinea, and it can be traced through Australia, from north to south into Tasmania. Such being the case, there is small room for surprise in the fact that many Tasmanian skulls can be found showing evidence of alien blood.

The prolonged sojourn of the Tasmanian in New Guinea, as I pointed out in 1916, produced, by his intercrossing with the earlier inhabitants, apparently Pre-Dravidian and Negrito,—the Papuan. About this there can be little room for doubt.

But for the fact that ethnical and somatological characters are so inextricably mixed up by anthropologists, we should never have had the contention that the natives of Torres Straits were "Papuans." Essentially they are Australians, though there is indubitable evidence of Negrito as well as of Polynesian blood among them.

My interpretation of the origin of the Papuan profoundly affects current views as to the "negroid" descent of the Oceanic "Negroes." For if I am right, these, and the "Negritos," can no longer be regarded as derivatives of the African Negro, unless we regard all as derived from some low, Pre-Dravidian stock, through the Tasmanian, or some near ally.

This must affect our conception of the "Melanesian." As at present understood the "Melanesian" is a compound of diverse cultures, and equally diverse racial intermixtures. It may well be that we shall, as I suspect, find the type of the Melanesian in the skulls brought back from Easter Island some years ago by Lord Crawford. These are now in the British Museum. When I came to examine them I found that they disclosed not the slightest trace of Polynesian blood; though Easter Island is supposed to be one of the fastnesses of the Polynesians. In this view I was confirmed by my colleague Captain Athol Joyce, who had come to the same conclusion on other grounds. Isolation from the rest of the Pacific kept these early settlers relatively pure.

The Australian, again, has been just as woefully misinterpreted. Our present conception of the aborigines of Australia is founded on general averages compounded mainly on superficial somatological characters, interpreted to harmonize with cultural characters. So far as craniology is concerned no more has been done than to take the averages of a number of indices. Progress in this direction is impossible. Any careful examination of a large series of skulls will show, clearly enough, that they contain evidence of considerable racial intermixture. They are to be regarded as of Pre-Dravidian descent, and the Pre-Dravidians had more than a little of Mousterian blood. It is time, then, that we realized that all skulls from Australia are not necessarily "Australian."

But let me return to the Polynesian. Dr. Haddon interprets the Polynesian as a derivative of the Himalayan, or "Mongolian" stock. "The Polynesians," he remarks in his most suggestive and stimulating "Races of Mankind," "are difficult "to place, as they are a composite people; on the whole they may be regarded as "mainly of Nesiot and Proto-Malay origin, but the 'Mongoloid' characters are very "attenuated, and there is probably another non-Mongoloid element which is very "brachycephalic: some have a Melanesian strain"

No race, I venture to think, has been more misunderstood than the Polynesian. Yet, as I have already remarked, the Polynesian skull is one of the most characteristic of all human skulls: and it is quite certain that it has nothing in common with the "Mongoloid" skull.

In the Report on skulls from New Guinea, to which I have already referred, I pointed out a striking likeness between this and skulls from Oregon and California. These, and the typical Polynesian skull, bear a most wonderful likeness to skulls from Buenos Aires ("La Tigra") and Arkansas, which have been described by Dr. Aleš Hrdlička. Their superimposed contours are admirably shown in Sir Arthur Keith's "Ancient Types of Man." The inference seems unavoidable, that the "Polynesian" is a migrant from America. This much was urged more than eighty years ago by Ellis; but that view never found favour with anthropologists.

Yet this interpretation seems more probable than that these fossil men from Oregon, California, Arkansas, and Buenos Aires found their way into America from the Pacific. From their antiquity it is extremely improbable that they would have had the means of making so tremendous a voyage. It seems much more likely that later descendants of these people—who, and this is significant, enjoyed a wide range in America—at a much later stage in their history migrated into the Pacific.

If my interpretation of this matter finds acceptance, a matter which has long troubled us will be cleared up. The common likeness between all these skulls, and it is a most surprisingly close likeness—closer indeed than that between any other known series of fossil or recent skulls—can be no mere coincidence: but implies a common origin. The suggestion, then, that the "Polynesian" originated in America cannot, I venture to think, be lightly dismissed. He is, in short, identical with my *H. sandvicensis*.

So far, I have had little opportunity of examining "Amerind" skulls. But in one particular I can confirm Dr. Haddon's comments on the Neo-Amerinds. In regard to the Tehuelche, he remarks "The Ona of eastern Tierra del Fuego are "probably a branch of the Tehuelche." It would rather seem that the case should be put the other way about. That is to say the Fuegian, by isolation, has retained his primitive character, while the Patagonian has been modified by intermixture with other races. One has but to compare the superimposed contours (Plate L., fig. 5) of the two to see that the Fuegian is evidently the purer bred.

The Fuegian skull has a quite characteristic form, and must be taken as the type of distinct species—*H. patagonicus*.

The much smaller skulls in the British Museum, from Bolivia and Peru, seem to be derivatives of this type, modified by alien blood yet to be traced. At the present moment I hesitate to accord them sub-specific rank.

One passes naturally from the consideration of the Amerinds to that of the Old World Asiatics. The Thibetans are commonly regarded as of mixed origin but the form of the skull and their isolated position seem to suggest that, like the Tasmanian and the Fuegian, they are really relics of a relatively pure stock, preserved by their relatively isolated position.

Dr. Haddon regards them as a "mixed race" of his Asiatic Xanthoderms. The "Bod-pa," he tells us, belong to his "Paroean" Group. But there is another, "longheaded, with nearly regular features, long, fairly well-shaped nose, with a "good bridge. Skulls from the eastern province of Khams, which evidently belong "to this element, are described as dolicho-mesocephalic, broad faced, rugged and "massive. They differ from the southern dolicho-cephals of South Asia, and "evidently represent an ancient stock, affinities to which may be looked for in "other marginal areas; they may, indeed, have been the first inhabitants of "Thibet."

A skull from the Hodgson Collection answers fairly well to this description. It is labelled "I'nu Bhotia, Transnivem," and I venture to accord it the rank of a species, *Homo himalayensis*. It is a very remarkable skull; rugged as to the face, with an excessively broad interorbital region. It is metopic, and has a singularly heavy basi-occipital.

As variants on this—I hesitate for the moment to give them the rank of sub-species—come the Limbu, Annamese, and Celebesian skulls.

The Limbu skulls in this collection are particularly interesting : affording ample confirmation of the value of the mode of cranial analysis which I am advocating here. What seems to me the type of the race is a skull with very sharply defined characters : such as to justify one in regarding it as a relatively pure type. The skulls in the collection, ascribed to Limbu, Sunwars, Lepcha and Kirantis are for the most part indubitably "Limbu"; but there are other nominally Limbu skulls which are emphatically of Gurkhas, and other alien types. They are "Limbus" only as a consequence of intermarriage between Limbu men and non-Limbu women.

So far I have been unable to examine any skulls from the actual neighbourhood of Thibet : and very few from Mongolia, China or Further India.

Dr. Haddon's Indonesians, or Nesiotics, and his Paroeans, are both to be traced to the Himalayas. The Annamese seem to form a clearly distinct type, from which the Burmese seems to have been derived : while both show unmistakeable likeness to *Homo himalayensis*. I have had, so far, no opportunity of examining skulls from Assam, or Siam, but they will probably prove to be of the same stock. Celebes seems to have produced a type of its own; but this, again, is traceable to the Himalayan just referred to. But of the craniology of Further India, as well as of China and Mongolia, we have yet much to learn.

I have no scheme to submit embracing the whole Classification of the Hominidæ. That, on the lines I have suggested, will be a task of years, and cannot be accomplished single-handed.

Convinced that our present methods of investigation are futile, I have made a beginning, which I submit for criticism.

We ought no longer to rest content with our efforts to arrive at a Classification of Man based on the results of "Average Indices" which, as Professor Fleure has so ably shown, are worse than useless, for they are misleading, defeating the very ends for which they were created. We must, in short, adopt the methods of the Zoologists, and throw our old traditions to the winds.

It has become an ingrained habit of mind among us to regard Post-Mousterian man as representing but a single species including a number of "Races," or, as some have it, "Somatological Units." Yet we should none of us insist on a similar standard of classification for the Apes, and Monkeys, which we include in the same sub-order as the Hominidae! It may be urged that the difference between, say, the Gorilla and the Marmoset are too great to make this feasible. We do not hesitate, in short, to divide them into genera and species. And the difference between the several species of, say, the Genus *Colobus*, are much less striking than the difference between the various "Races" of the Genus *Homo*; which include types showing striking contrasts, both superficial and skeletal. Or compare our treatment of the Genus *Homo* with that of the Zoologists in regard to the Genus *Felis*, or the Genus *Fringilla*.

By their means, and their means alone, shall we be able to trace out the affinities and descent of the various types of Man, which we all profess to be striving to discover. How much longer are we going to flounder about in a morass?

In my paper in MAN, already referred to, I described four species and one sub-species of the Genus *Homo*. I now add two further species—the Papuan and Himalayan. The diagnoses of these and the figures of those already described, which were not figured in my Report on Skulls from New Guinea, will be found at the end of this paper.

(To be continued.)

Australia: Technology.

Basedow.

Slate Scraping Implements of the extinct Adelaide Tribe. By **106**
Herbert Basedow, M.A., M.D., B.Sc.

It seems a rather remarkable coincidence that after eighty-eight years of European settlement in South Australia, two men should, independently and without any knowledge of each other's intention, have simultaneously drawn attention to an unrecorded stone implement which in days gone by was in all probability familiar to many colonists. I refer to the slate scrapers of tribes originally inhabiting the Adelaide plains which were recently described by Dr. T. D. Campbell* and the writer.†

Having perused Dr. Campbell's article, I feel that a few additional notes upon the subject would not be out of place, especially as the views expressed by the other observer regarding the purpose of these objects are entirely opposed to my own.

Dr. Campbell describes three specimens, all more or less fractured, from south of Port Noarlunga. I have twenty-one before me, the majority of which are complete and were collected at Normanville, some miles south of the previous locality; two were found at Woodville, west of Adelaide.

* "An Account of a Hitherto Unrecorded Type of Aboriginal Stone Object," by T. D. Campbell, D.D.Sc., *Trans. Roy. Soc. S.A.* 1924. Vol. XLVIII, pp. 74-78.

† "The Australian Aboriginal," Adelaide, 1925, p. 366, Plate XLIII.

The specimens are either blue, brown, or banded clay-slate. The general form is a flat segment, approximately equal to or greater than half a circle, the two corners at which the arc meets the chord being rounded off. The edge representing the chord is either straight or notched at the centre. The circular edge of the object is bluntly "shaped," while the straight or grooved edge is invariably sharpened by grinding or scraping.

Dr. Campbell draws attention to the last-named, important feature without claiming any particular significance for it. He writes: "Each possesses a large "notch on its lower border which was deliberately cut out, as is shown by the "smoothing down of, and remaining scratches adjacent to, the edge of the notch."

The length of the objects varies from 7·5 to 12·5 centimeters; the width is also variable, but in the larger specimens is usually between 7 and 8 centimeters. The maximum thickness observed in any of the slabs in my possession is 1·25 centimeters.

The flat surfaces of the specimens are roughly smoothed by a process of rubbing or rasping, in consequence of which a number of irregular furrows and scratches have remained. In one or two instances these markings are more symmetrical and may represent an intentional pattern.

In the absence of any explanation, of literature on the subject, and of any survivors of the tribes, Dr. Campbell suggests that the specimens might be sacred objects akin to the stone "churinga" of Central Australia, mentioning the following as the parallel features: "they are made from a thin palette of stone, and present "obvious indications of having been shaped into definite forms; they have "incised markings, and show the application of red ochre."

None of these characteristics, however, is proof of the sacred purpose suggested. For instance, the pubic appendages of pearl shell used by the Northern and Northwestern coastal tribes are shaped, carved, and red-ochred, but no one would suggest that they are ever used as "churingas."

In making a comparison between the newly-recorded objects and stone "churingas" from Central Australia, Dr. Campbell remarks that "although the "reniform contour of the former differs from the usual oval or pear shape of the "latter, this does not totally destroy their likeness, but renders it the more "interesting."

I do not attach much importance to the incised markings on the objects, which in most instances are without any intended order or symmetry, and of the nature of simple scratches formed during the shaping processes. Dr. Campbell concurs that these incisions are "crude and asymmetrical," but favours the idea that they are decorative, and that they show "some impressive points of similarity" with those on genuine "churingas" from other parts of Australia. "Probably the "most striking and readily interpretable," he writes, "are the 'arrow' figures. ". . . These are identical with the figures present on some churingas which "are attributed to natives of an emu totem in the Central tribes; these 'arrow' "figures represent the footprints of the emu, or some animal like the kangaroo "or rat kangaroo."

A single-lined "arrow" figure is always intended to represent a bird-track; when the central line is doubled, it stands for that of a marsupial like the kangaroo; and in many cases a pair of short parallel lines without any lateral embellishments answers the same purpose as the latter. It is by no means the rule, however, that "churingas" bearing emu or kangaroo tracks necessarily belong to an individual of either the emu or kangaroo totem; or conversely, that "churingas" of the emu or kangaroo totem always have the above mentioned tracks cut upon them. As a matter of fact bird-tracks are comparatively scarce among the markings upon

"churingas" in Central Australia, except upon the sacred sticks used by the rain-makers. In that case, however, they represent the tracks of the plover.

In the book previously referred to,* I have included a complete "churinga" pattern of the kangaroo totem which to the uninitiated would not be decipherable because it does not include any reproduction of tracks or other obvious characteristics.



FIG. 1.—MALE NATIVES OF THE MURRAY RIVER TRIBE, SOUTH AUSTRALIA. Both are wearing opossum-skin rugs, that of the man on the right showing a decoration of parallel zig-zag lines of property markings. The other man is holding the characteristic shield and small spear-thrower which were used by the now extinct tribe.

ventional markings or patterns, of which each was peculiar to and recognised in the sense of a brand, or property-mark.

Some of such markings are seen on the rug of the native on the right-hand side of the accompanying photograph (Fig. 1), consisting of a series of parallel zig-zag lines of white upon a dark background.

In regard to the purpose of these objects, Dr. Campbell writes: "The possibility of their being a form of implement can be readily dismissed, as they are of a material and shape which would serve no utilitarian purpose." This deduction is, however, not in accordance with the real facts of the case—the objects were used as a special type of scraper.

My information comes from an old aboriginal of the River Murray tribe, and is reliable. We know, on the authority of Dr. M. Moorhouse,† that the natives of the district in which these implements are principally found—namely, "those from the southern coast and Rapid Bay—serve as a connecting link between the natives of Adelaide and those of the Murray River, as they speak both languages." Hence the use of this implement would formerly have been known to all tribes between Adelaide and the River Murray.

* Figure 49 *et* page 352.

† For reproductions of particular designs, see A. W. Howitt, "The Native Tribes of South-east Australia." London, 1904, fig. 50 (page 742); and A. van Gennep: "Dessins sur peaux d'opossum australiennes," Rijks Ethnographisch Museum te Leiden. 1907, Plates I-XIV.

‡ "First Report of Protector of Aborigines, 1839—Papers Relative to South Australia" By Authority. London, 1843, page 321.

Emu and kangaroo tracks appear more frequently upon weapons, domestic utensils, and other belongings of the natives. I regard the more regular and distinctive markings upon a few of the objects here under discussion as entirely personal. It is a well-known fact that the tribes of the River Murray and others of South-eastern Australia used to decorate the skin rugs† they wore, and their personal effects generally, with con-

The implements were exclusively used for scraping the fat and fleshy tissue from the opossum skins, which the natives subsequently sewed together into rugs with kangaroo sinew. (See Fig. 2.)

The method employed was as follows: The freshly removed skin was laid, fur downward, over a cylindrical rod and drawn tightly around it with the fingers of the left hand. The implement was then gripped by the opposite hand in such a way that the convex edge was against the palm and the flat surfaces between the fingers and thumb. Holding the rod in a vertical position, the concave (or straight) cutting edge was placed against the skin over the rod and worked at an angle downwards, the cutting edge shaving off all adherent pieces of fat and other soft tissues in doing so. The position of the skin, relative to the rod, was frequently changed and the process continued until the whole inner surface of the pelt had been prepared and cleaned in a similar way.

The advantage of a concave cutting edge obviously was that by an accommodation of the two curves, presented by the implement and the rod, respectively, a greater area of skin was scraped with every downward movement of the hand; and the process was performed without so much risk of cutting the skin as would have been the case with the ordinary convex or straight-edged stone scrapers on a plane surface. This being the one and only use to which the implements were put, and considering the delicate nature of a freshly obtained opossum skin, the comparatively blunt and soft edge of slate would naturally have been preferred to the hard, sharp, and splintery edge of sandstone or quartzite.

HERBERT BASEDOW.

America, North: Archæology.

Smith.

A Semi-subterranean House-site in the Bella Coola Indian Area on the Coast of British Columbia. By Harlan I. Smith.

107

There is a semi-subterranean house-site, consisting of a rectangular excavation surrounded by an embankment, on Joshua Moody's clearing on the Indian reservation at Bella Coola, B.C. I found this site, June 13, 1924, having been told of its approximate location by Mr. T. F. MacIlwraith, Mr. Iver Fougner, Indian Agent, and Ahschimlic. It is in the angle between the main valley highway and the old road to the Indian village, distant 185 feet at right angles north-west from the middle of the old road, at a point thirty feet north-east from the first small bridge on this road or 685 feet from the centre of the main highway. Within a hundred feet south-west of the site or one hundred and eighty feet south is a creek or slough, the one crossed by the



FIG. 2.—SLATE SCRAPING IMPLEMENT, showing how it was used by the extinct Adelaide Tribe to prepare opossum or kangaroo-skins for rug-making.

above mentioned bridge. In this the water is about six inches deep and flows north-west. It is luxuriant with skunk-cabbage.

The rectangular excavation is $17\frac{1}{2}$ feet long by $12\frac{1}{2}$ feet wide by two feet deep, and longest north-east and south-west. The surrounding embankment varies in width from 5 to 10 feet, is one foot higher than the natural surface of the soil outside the house-site or three feet higher than the floor of the excavation.

The antiquity of this house-site may be suggested by the fact that a hemlock tree standing in the south-eastern part of the floor is over two feet in diameter. Joshua had dug in the bottom of the floor from a spirit of exploration, and in the excavation he had made I noted broken and crackled stones, such as are formed by the Bella Coola method of boiling by dropping heated stones into a box containing whatever was to be boiled.

Four inches below the surface, at the north-western corner of the floor, the soil was clean, appearing to be natural and undisturbed. HARLAN I. SMITH.

Africa, South : Technology.

Laidler.

Note on a globular Bead found in a Bushman Shelter, near Aicedale, Cape Province, South Africa. By P. W. Laidler, F.S.A. 108

When with Mr. Hewitt, of the Albany Museum, excavating a Bushman shelter on the farm Wilton, near Aicedale, C.P., by the permission of the owner, Mr. Wilmot, I found a globular bead of cobalt blue glass, multifaceted (eight facets), facets triangular and placed alternately (VVV), a raised line separating each facet. It is covered with a heavy grey patina. It was found at a depth of two feet from the surface, and towards the entrance of the shelter, in vegetable ash, and associated with Bushman implements of the usual inland cave-shelter type.

This bead is identical with a bead that I found some years ago beneath a portion of fallen wall, five feet beneath the surface, in the Roman Camp at Piercebridge, North Yorks., and it is the only article of non-Bushman culture so far found in this shelter. There is no stratification by which the date of this deposit may be gauged. But on the walls of the shelter, formed by overhanging ledges of rock, are paintings of fat-tailed sheep, which were introduced into South Africa by the Hottentot. This deposit is, therefore, in greater part not anterior to *circa* A.D. 1200. It was in occupation up to the time of the European settlement, and the district is mentioned by Barrow as one of the last strongholds of the Bushmen. The kloof in which the shelter is situated is not easy of access. It is not likely that the bead was introduced by the agency of the 1820 settlers, nor is there any record of a collection in the Cape from which this bead might have found its way. In my opinion this bead has travelled from some Roman site in North Africa to this Bushman shelter through casual change of possessor, of varied nationality, being in the latter, and probably greater, portion of its travels a treasured ornament of Bushmen.

P. W. LAIDLER.

Religion.

Jones.

Catching the Sun. By J. J. Jones.

109

In MAN, 1923, 144, Mr. A. M. Hocart seeks to explain a Fiji custom whereby travellers seek to stay the sun in its course in order that they may arrive at their destination before sunset. The custom is given by Frazer (G.B. I³, p. 316). "On a small hill in Fiji grows a patch of reeds, and travellers who feared to be belated used to tie the tops of a handful of reeds together to prevent the sun from going down." Mr. Hocart rightly protests against a too ready application of the categories of sympathetic magic, animism, and the like to primitive

customs. These categories may classify but do not always explain. But it may be doubted whether he has succeeded in explaining this particular custom in terms of something simpler than sympathetic magic. He says that "before tying the 'reeds, the traveller makes signs to the sun, an action called *yalovaki*, from *yalo*, 'soul, image, double,'" that is to say, before the reeds are bound a spell is uttered by which the reeds become the double of the sun, or in other words the sun is brought down to the reeds. If, therefore, the reeds are tied together, so will the sun be tied and hindered in its course.

Mr. Hocart says that we have here "a logical application of a definite theory of 'doubles.'" The reeds, that is, are the second self of the sun, and what is done to the second self is likewise done to the original self. But are we not here back with sympathetic magic again? The two selves are nothing if not sympathetic. What Mr. Hocart has done is to attempt to explain why or how these reeds and the sun come to have this sympathy; and in any case, it is not clear that the theory of doubles is a simpler explanation than the theory of sympathetic magic. It may, indeed, be just so much of an improvement on it as it is nearer to particulars. But cannot we get nearer still to particulars? It is unsafe to credit primitive man with such a mental achievement as a theory of doubles. But we are on perfectly safe grounds in assuming that he like all men observes things first and draws his conclusions afterwards. When, then, we describe any actual practice as an instance of sympathetic magic, we do not mean that primitive man has formulated a general theory that what is done to one thing is suffered by that which is related to it, but only that in any particular instance two things have been observed to be in such close relations as to become, not the one the double of the other, but actually identical. Identity is the maximum of similarity, but primitive man does not stop to think whether that maximum always exists. His observation takes it for granted, however, that it does, and it is on this observation that he acts, and not on any law of sympathetic magic. To explain these magical practices, then, what is needed is not to look for any general theory that primitive man has reached, but to attempt in each case to discover what the observation of fact was on which the practice was based. That is not always possible, for custom may have become so elaborated as to hide the original observation from view, or the latter may have been one singular and striking occurrence, and its very singularity prevents us from tracing it. Then there is a large class of magical practices, of which leaping into the air to make crops grow is a type, which cannot possibly be based on experience of observation.

But in a very large number of instances it is possible to discover what led the savage to think that any two things were in sympathy. In this particular rite of catching the sun it would be interesting to know more about these reeds. Why should they rather than any other plant be made the double of the sun? Were they in any way so peculiarly responsive to the light of the sun or to its course in the sky that man could not fail to observe it? If they were, then they were already a double of the sun, that is in sympathy with it, without any spell being needed to make them so. The spell would be a later development and would serve to perpetuate the custom even in face of the experience that it did not stay the sun. When that happened the blame could be thrown on the spell. The travellers would try and find out what mistake they committed in making the spell; they would not at once admit that the whole rite was utterly unsupported by experience.

A magical practice mentioned by Pliny affords a close parallel to the Fijian custom, and at the same time proves how large a part was played by observation in originating such practices. Of the plant heliotrope he has this to say (*Nat. Hist.* 2, 41, 108-9)—*Miretur hoc, qui non observet cotidiano experimento, herbam unam,*

quae vocatur heliotropium, abeuntem solem intueri semper, omnibusque horis cum eo verti, vel nubilo obumbrante—"This is a fact to excite wonder in a man " who has not noticed it in his everyday experience, that one plant which is " called heliotrope always faces the sun as it moves along and turns round with it " at all hours, even when a cloudy sky obscures it." The heliotrope is therefore observed to be in close sympathy with the sun, and it remains to see whether there is any practice based on this observation. In another passage of his *Naturalis Historia* (22, 21 (29), 61), Pliny says " Magi heliotropium quartanis quater, in- " tertianis ter alligari ab ipso aegro, precari eum soluturum se nodos liberatum, " et facere non exempta herba. The Magi (prescribe) that the heliotrope should " be tied four times (= ? tied with four knots) in the case of quartan, and three " times in the case of tertian, fevers, by the patient himself, and that he should " vow that he will undo the knots when cured; he is to do this without plucking " the plant." We may suppose that fevers were deemed to be the result of having within one too much of the sun's heat; the Malays, for example, think that a bright glow at sunset may throw a weak person into a fever (Frazer G.B³., I, p. 319). To tie up the heliotrope, therefore, is to check the sun and stop him from sending too much heat into your body.

J. J. JONES.

Obituary.

Childe.

Dr. Ferencz Laszlo. By V. Gordon Childe.

110

In the death of Dr. Ferencz Laszlo, of the Szekel National Museum at Sepsi Szent György, European prehistory has sustained a very grave loss. His work, although not widely known outside Hungary, belonged none the less to the domain of world history. In the rich metalliferous region of the Upper Alt he had been patiently exploring a brilliant Copper Age civilisation the relics of which, notably the magnificent painted vases, the clay plastic and the substantial houses with their painted plaster decorations, not only betray unmistakable affinities with the Thessalian, but also offer curious analogies to material from the Ancient East. The station of Erösd which Dr. Laszlo excavated with such meticulous care provides, as a result of his labours, our most precious source of information on the culture with painted pottery in south-eastern Europe. He was actually engaged in further researches on the site at the time of his sudden death, which further has cut short the publication of his earlier results. The publication of his manuscript, owing to the separation of Transylvania from Hungary, has become a task for foreign science and would constitute the most fitting memorial to this most modest but most careful savant.

V. GORDON CHILDE.

REVIEWS.

Ethnology: Trepanning.

Wölfel.

Die Trepanation: Studien über Zusammenhänge und kulturelle Zugehörigkeit der Trepanation. I. By Dr. W. J. Wölfel. *Anthropos*, xx., pp. 50. 1925 (separate).

The practice of trepanning has a peculiar and discontinuous distribution. In prehistoric times it was practised all over Europe, in the Atlas region of Africa, and in the Canaries; and it persists in Albania and Serbia, among the Berbers, and apparently, also, in Abyssinia. Outside Europe it is well attested in New Guinea and Melanesia, throughout Andean South America, in Mexico, and in the Mississippi basin. Broca, writing in 1876, attributed these widespread occurrences to independent inventions, but had only unfounded beliefs about the motive of the practice. Dr. Wölfel, trained in the "Kulturhistorische Methode," and utilising, for Melanesia,

a number of detailed descriptions, by eyewitnesses, of the practice itself and the purpose of it, proposes an alternative solution, and raises a number of interesting questions by the way.

Realising that it is only by a comparative method, and with clear ideas as to the origin and purpose of trepanation, that any progress can be made, he sets out from the Melanesian examples, of which alone we have contemporary explanation. Here trepanation is a piece of normal war-surgery, to remedy skull-wounds from sling-stones or clubs: it is deftly and intelligently performed, with surprisingly complete precautions against septic trouble, and is frequently successful. There is a magical supplement, to be sure, which is regarded as essential; but it has no more to do with the efficacy of this kind of surgery than has the colour of the overalls of a London operator. Occasionally, in Melanesia, trepanning is used to relieve pains in the head not due to wounds; but this extension of usage hardly needs explanation, especially as some painful disorders of the brain are accompanied by paralysis of limbs and other symptoms common to these and to severe head-wounds.

In America, there is no such eyewitness record as in Melanesia, but the numerous trepanned skulls from sites all through the Andean region, in Mexico, and in the "mound-building" region of North America as far north as Lake Huron, show very close similarities of procedure. Often it is clear that trepanning was performed to mend heads broken by clubs or sling-stones. But in many cases there is no clear evidence of a wound, and in some there are traces of disease; so that the twofold use of the operation, as in Melanesia, may safely be inferred. There are also a few American skulls which have been trepanned after wounds described as "sabre-cuts." If these have been correctly diagnosed, Dr. Wölfel's generalisation that trepanning stands in essential connexion with wounds from clubs or sling-stones does not seem to be well founded, unless he can show that these examples belong to a separate and subsequent category. But this he has omitted to do, and his comments on McGee's necessary pioneer work in classifying the kinds of operation covered by the general name of "trepanation" indicate that he has not realised the need for this precaution—and also for fuller discussion of the traces of cauterisation noted by MacCurdy—if the rest of his argument is to stand.

Proceeding by the "Kulturhistorische Methode" (which its English exponents have so distressingly mistranslated as the "historical" method), Dr. Wölfel seeks next for other "elements" in the cultures of Melanesia and the Americans respectively, which coexist with trepanation in both regions. Allowing for imperfections in the records at present available, he makes out a good case for approximately similar distributions of the use of slings, and of stone-headed clubs; also for the practice of carrying the sling coiled round the head—some might prefer to say that in the first instance a head-cord was used for a sling; but this would savour of "convergence"—and for the association of all these with the "Two-Class-Culture" of Gräbner and Pater Schmidt; at all events in the Bismarck Archipelago the club-using people are inlanders, and inferred to be of older origin than the maritime population. This further association, however, is not formally demonstrated by Dr. Wölfel, and is only noted here as a signal of the trend of his argument. He is himself quite frank about the lamentable imperfection of our present knowledge of slings and their distribution. Stone clubs, of course, have been greedily collected, and carefully studied both by mere evolutionists and by votaries of the "Kulturhistorische Methode": Dr. Wölfel promises a separate monograph about them later on.

In America, though the denial of pre-Columbian slinging in the "Handbook of the American Indians North of Mexico" is to be revised in view of the Vinland slingers described in "Eric the Red," and of the "demon-head" slings of Algonkin

warriors and other peoples, the distributions do not fit quite so neatly. An Eskimo legend believed to refer to trepanning is not quite the same thing as a trepanned Eskimo skull; and short of this there seem to be no trepanned skulls in America north of Lake Huron, nor outside the range of the "mound-building" culture; yet there are slings among the Beothuks, the Kwakiutl, the Twana in Washington State, in California and on the Gulf Coast and widely in Central American regions, where trepanning is at present unrecorded. In fact, the two distributions do not seem to "cover" at all, in the sense claimed by Dr. Wölfel. He has also not improved his case by including among American "stone clubs" those with grooved and lashed heads, as well as perforated heads like those of New Guinea and Melanesia. Nor does he offer in America, any more than in Melanesia, formal proof of his claim that clubs and slings cohere with the "Two-Class Culture" and with the cultivation of maize; and the distribution of clubs, like that of slings (as he gives it here) is a good deal wider than is consistent with such coherency.

The conclusion which Dr. Wölfel draws from the considerations briefly summarised above—that Melanesia and New Guinea on the one hand, and the trepanning peoples of America on the other, have enjoyed one and the same culture-complex of trepanning—slinging—clubbing elements—will have been anticipated long ago by the intelligent reader. But has he proved his point?

His own argument presumes that trepanning owes its vogue to the prevalence of a particular kind of head-wound; and, as he says, for stabbing-wounds in the head trepanation is of no avail. The coherence which he claims to have proved is not, therefore, a coherence between independent "culture elements" but between cause and cure of a particular kind of wound; and it is difficult for a mere evolutionist to be convinced by the theory that only on one side of the Pacific did human ingenuity discover that if a skull was indented past repair the best remedy was to clean up the mess by removing splinters, with simple aseptic precautions, and otherwise leave Nature to recover the patient. And in regard to the clubs, in particular, the fact that not only stone heads for them, but a whole "formentafel" of round, square, pointed, star-rayed, and multi-corrugated types occur in both regions would seem to weaken rather than strengthen the argument for diffusion; for if the shaping of a club-head to a round or square or any other shape could only be achieved originally, once and in a single region, the probability that each and every such phase in the original "formentafel" was escorted safely into the trans-Pacific area of this missionary enterprise, decreases rapidly with each additional variety. The occurrence of star-headed clubs on the Atlantic seaboard of Brazil is ingeniously explained by the migrations of the "Andean" Tupi people: but, if the distributions "cover," as Dr. Wölfel says, where are the Brazilian examples of trepanning? There are also the little problems of the American "sabre-cuts" and "cauterisations" which are regrettably absent from the Melanesian record. Can it be that persons so incapable of making a square or star-shaped club had brain-waves leading them to invent slashing weapons, or to sterilise by cautery instead of coco-nut water? In the name of all that is "methodisch" and "kultur-historisch," what has become of the "method of difference"?

For interpretation of the prehistoric European trepannings we must wait for Dr. Wölfel's next instalment. Of its general trend, however, he has given us a foretaste, when he quotes with approval Menghin's claim that in the same way as the "Two-Class Culture" "adopted" maize as its cereal crop when it reached the Americans, so it "adopted" barley when it was diffused over Europe as the *Kultur des älteren Mutterrechts*. No doubt when he comes to the special case of Attica he will know how to associate the motherless offspring of the broken head of Zeus with her "best of gifts," the olive-tree: from *Tritogeneia* to *Trētogeneia* it is a shorter step than across the Pacific.

J. L. M.

India : Folklore.

Edgerton.

The Panchatantra Reconstructed . . . Text, Critical Apparatus, Introductions, Translation. By Franklin Edgerton. Two volumes. American Oriental Society, New Haven, Conn. 1924. London : Milford. 36s. net. $9\frac{1}{4} \times 6\frac{1}{2}$ xix $\times 408$ and 405 pp.

Owing, perhaps, to its enormous popularity calling for continuous versions in different parts of India and its translation into nearly every conceivable language, the original version of the Panchatantra is lost. Scholars have contented themselves with tracing its history as far as data allowed, or specialising on some particular recension. In the present work, however, Professor Edgerton attempts the ambitious task of reconstructing the lost original Sanskrit text by a minute comparison of the most important versions. Such a method necessitates much conjecture, but this factor does not really detract from the value of so interesting an enterprise. The amount of work involved and the feeling that one would be attempting the impossible has doubtless kept scholars, less persevering than Professor Edgerton, from following the same line of research.

The present work forms Vols. ii and iii of the American Oriental Series, and is published by the American Oriental Society of New Haven, Conn. Vol. i contains the text and critical apparatus printed one above the other on each page. The text forms the suggested original reconstructed from a comparison of the principal extant versions, while the critical apparatus shows exactly what the different readings are, how and why a particular one agrees or disagrees with the other and what is the resulting reading to be adopted as nearest the original. Any doubtful lines, words, or even portions of words are clearly indicated, so that the methods employed are most exhaustive. It is, however, imperative to read very carefully pp. ix-xix, which deal with the typographical devices and abbreviations used throughout. Once this has been mastered all is plain sailing. Professor Edgerton is to be congratulated, not merely for the ten years of untiring labour and unremitting care he has bestowed on his work, but for the clear way in which he leads us step by step through every section of his work. The materials are marshalled in due order, the method of comparative criticism and deduction is described, with the result that we can see exactly how and why he reaches his final conclusions. Whether the evidence warrants such conclusions is for scholars in Sanskrit textual criticism to decide.

I now pass on to Vol. ii, which will naturally appeal to a far wider circle of readers. This gives full details of materials available and the methods adopted in the reconstruction. Secondary inter-relationships of the various versions are discussed, Hertel's views are criticised, and tables of the stories and the text-units of the original are appended. Finally, we have the complete English translation of the reconstructed Panchatantra. Space will not permit the examination of the above sections in any detail. It is only possible to touch briefly on what strike me as the most important points to be noticed.

The versions selected for the reconstruction of the original *Pañchatantra* are those which together contain practically all available data. The actual versions employed are : *Tantrākhyāyika*, *Southern Panchatantra*, Nepalese *Pañchatantra*, *Hitopadeśa*, the poetic versions in the *Kathā Sarit Sāgara* and the *Bṛihat Kathā Mañjari*, the "Textus Simplicior," *Pūrṇabhadra*, and the principal offshoots of the Pahlavi translation. The texts are then divided up into the smallest possible units, thus facilitating a comparison between the materials in the different versions as far as their meaning is concerned. Then the question of relationship is studied. When Professor Edgerton found a "sentence or verse in identical or practically identical language, and in the same position, in all the prose Sanskrit recensions, " and when its general sense was found in the poetic and translated recensions "

he assumed that it was a literal inheritance from the original. The primary results claimed from this investigation are fully stated on pp. 8, 9 and cannot be given here.

The most interesting point to be considered is the extent of divergence from the results arrived at by Professor Hertel, who has spent so many years in studying the different versions of the Panchatantra. (See especially his *Tantrākhyāyika*, 1909, and *Das Pañcatantra*, 1914.)

Professor Edgerton considers there are four independent streams of Panchatantra tradition, the *Tantrākhyāyika*, Southern, *Brihat Kathā*, and Pahlavi versions. Professor Hertel, on the other hand, believes there are only two, the *Tantrākhyāyika* and "k," archetype of all the other versions. He would further derive *all* existing versions from a corrupt archetype called "t." Professor Edgerton considers both "k" and "t" are as fictitious as Damanaka and Karatāka themselves. There are many other points in which the two scholars differ, most of which are made clear by comparing their respective genealogical tables (Edgerton, Vol. ii, p. 48, and Hertel, *Das Pañcatantra*, p. 426).

In conclusion I would mention the evidence Professor Edgerton has gathered from his reconstruction with regard to certain questions about the original work. Its name was undoubtedly *Panchatantra*, and the meaning "Five Tantras." It is hard to say exactly what a *tantra* is, but it is probably simply a "book," or division of a literary work. With regard to its date, no exact year can be assigned. It is post-Christian and must, of course, antedate the Pahlavi translation made in the sixth century A.D. This is as far as Professor Edgerton ventures to go. Hertel now brings the date about 300 A.D., but his evidence on this point is not conclusive.

The authorship is quite unknown. He was doubtless an orthodox Hindu, but further statement becomes little more than guesswork.

Hertel would put the home of the work in Kashmir, thus making both the *Panchatantra*, the *Kathā Sarit Sāgara* and the *Brihat Kathā Mañjarī* emanate from the same place. Professor Edgerton, on the other hand, places it in the south, perhaps the south-west of India, though he looks for further evidence before making any definite statement.

The work was written in Sanskrit. It was intended as a political textbook, but is *prima facie*, a book of stories of a story-teller, and if any particular tale lacks an obvious political moral it need not be rejected as part of the original work.

The above remarks, sketchy though they be, will help to show, I trust, the importance of this work. As a fine example of patient Oriental scholarship, it must receive our unreserved congratulation. Those who consider the task of reconstructing the original Panchatantra as impossible, or who disagree with many of Professor Edgerton's conclusions, must, nevertheless agree that the attempt was well worth the while.

N. M. PENZER.

Psycho-Analysis

Hocart.

Psycho-Analysis or Anthropology.

To the Editor of MAN.

113

SIR,—I regret that I should have given Dr. Ernest Jones (MAN, 1925, 27) the impression that I was trying to "score off" him, by seizing upon one little slip such as we are all liable to. Such fault-finding would be ungenerous towards a school of psychology which has, with all its errors of enthusiasm, rendered the greatest service to our knowledge of man. But it makes it all the more necessary to remind psycho-analysts of the limitations of their methods, and that they cannot, any more than the old school of psychologists, just by looking at a word or custom immediately tell us the mental processes that have given rise to it. Yet that is

what the psycho-analyst is constantly trying to do, as if the old school had not lamentably failed before in the same attempt.

Rivers once asked me what I thought of his "Dreams and Primitive Culture." "I don't like it," I told him. He asked me why. "Because," I said, "you take isolated customs, the history of which is quite unknown, and proceed to tell us how they came about. You say the use of a coconut as a substitute for a head is a case of condensation. How do you know? We do not know the various stages of this custom, then how can you say it is a case of condensation: it may have had a very different history." "But," he objected, "we have examples from the Mediterranean where we have all the facts." "Then why," said I, "go to the New Hebrides, when we have all the facts near home?" My question still remains unanswered. So long as the psycho-analyst takes his examples from non-historical regions the historian cannot convince him of the error of his ways, because he does not know the true origin so as to oppose it to pure speculation; he has to wait till the psycho-analyst comes home and works on well known facts to show him how uncertain his method is. Dr. Wohlgemuth has produced another example, very much better than mine; test cases would be much easier to collect if historians were not so unduly distrustful of the psycho-analyst as completely to ignore him. This is an unfortunate state of affairs, since it is only by the co-operation of history and psychology that we can arrive at the laws that govern the growth of human society.

I have elsewhere set forth what I believe to be the historical development of an idea, and invited the psycho-analyst to contribute his part (*Ceylon Journal of Science*, Vol. I, pp. 39 ff.). It is the idea of the mother-earth. The psycho-analyst would just take the idea and psycho-analyse it straight off the reel: here is a great field for the Oedipus and other complexes; but a comparative study of records suggests that the idea of the earth as a woman arose through the queen having the form or double of the earth put into her so that she became identical with the earth and her fertilisation would react on the crops. The idea of a woman and the earth, in themselves so dissimilar, were thus brought together. This personification attained to a great vogue and here it is that the psychologist comes in: he can explain the extraordinary appeal made to the human mind by the idea of mother-earth, an idea so pathetically expressed in the Rig-Veda. Here is an opportunity for co-operation; will it be seized?

Yours faithfully,

A. M. HOCART.

Africa, West: Ethnography.

Talbot.

Clitoridectomy in West Africa.

To the Editor of MAN.

SIR,—Mr. Malcom's statement in MAN, 69, 1925, "it is believed that the operation" of clitoridectomy "has the effect of making the girl sterile," gives a new reason for this practice and one that is surprising when it is considered that no people in that neighbourhood—and particularly the Ibibio, of whom the Efik are a branch—would willingly go against what is perhaps their principal aim in life, viz. to have as many children as possible. Various reasons are assigned for the ceremony in question, such as the idea that it is more cleanly or a desire to diminish the sexual feeling, but it is usually admitted that it is merely an old custom, the origin of which has been forgotten.

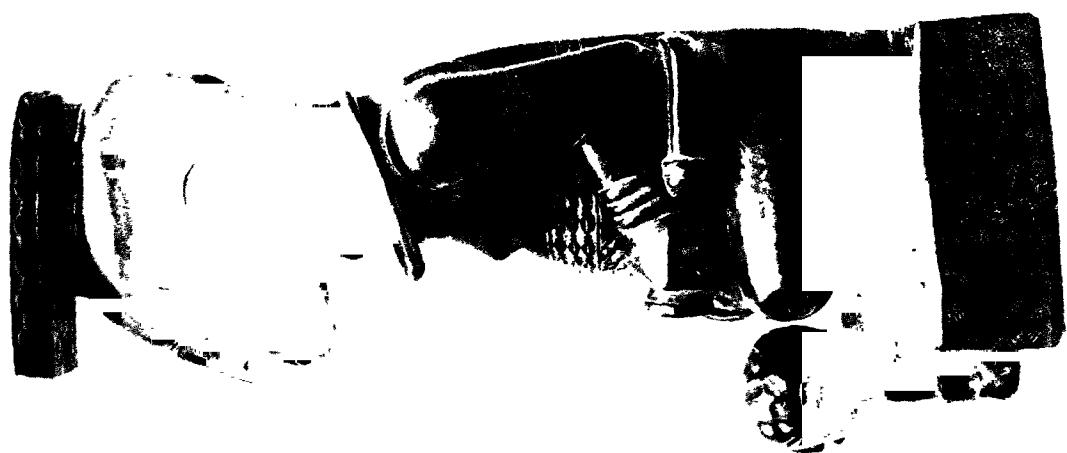
Yours, etc.,

P. AMAURY TALBOT.

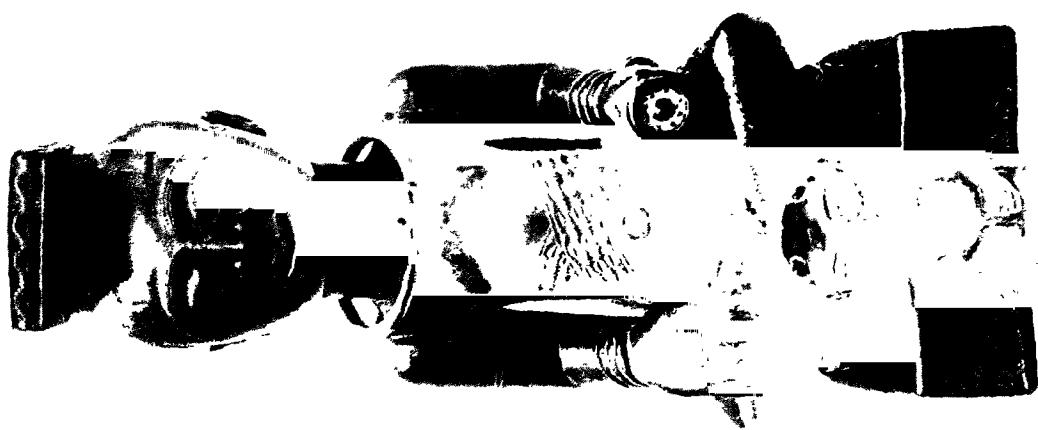


MAN, DECEMBER, 1925.

PLATE M.



THE PORTRAIT-STATUE OF MIKOPE MBULA, 110TH PARAMOUNT CHIEF OF THE BUSHONGO.



ORIGINAL ARTICLES.

With Plate M.

Africa, West: Art.

Joyce.

The Portrait-statue of Mikope Mbula, 110th Paramount Chief of the Bushongo. By T. A. Joyce, M.A.

115

It will be remembered that the expedition of Mr. E. Torday to the Kasai and Kwilu regions of the Belgian Congo in 1907-1909 brought back the first comprehensive account of that very remarkable tribe, the Bushongo (popularly known as Bakuba), who inhabit the territory south-east of the Kasai-Sankuru confluence. The intricate organisation of this tribe, their history, preserved by oral tradition through more than a hundred reigns, and their skill in arts and crafts, are given in detail in the monograph published by the Musée du Congo Belge at Tervueren, "Les Bushongo" (*Anthropologie*, Série III, Tome II, Fasc. I (1910)).

Among the products of native art brought back by Mr. Torday were four portrait-statues of past chiefs, the most important of which, the effigy of the great tribal hero, Shamba Bolongongo (early seventeenth century), was published in MAN, 1910, 1. Shamba was the ninety-third in the order of chiefs as preserved in tradition; the other three portrait-statues represented Misha Pelenge Che, Bope Pelenge I and Kata Mbula, respectively the 107th, 108th and 109th in the list of chieftains. Of these, the first two are illustrated in the Tervueren publication mentioned above, and the originals, together with the figure of Shamba, are now in the British Museum. The statue of Kata Mbula, which is practically identical with that of Mishe Pelenge Che, is in the Musée du Congo Belge. Native report states that it was not every chief who had his portrait carved, but only those chiefs in whose reign an artist of sufficient calibre was discovered. Artists of this nature seem to have been few. Shamba's statue was the first ever carved, but none exist, or are reported, until the last three mentioned (fourteen reigns later), and these three are said to be the handiwork of the same craftsman.

It is, therefore, a matter of great interest that a fifth should have recently come to light. This is the effigy of Mikope Mbula, 110th paramount chief of the Bushongo, and successor to Kata Mbula, whose statue was the latest of the four obtained by Mr. Torday. It has been acquired by the energetic Director of the Musée du Congo Belge, M. le Baron de Haulleville, who has, with characteristic generosity, sent me photographs, and accorded me the privilege of publishing them in MAN.

A glance at Plate M will show that the statue of Mikope Mbula conforms to the traditional pattern. It is carved from solid, hard, reddish wood, is some 62.8 cm. in height, while the base is 19.5 cm. broad by 17.0 cm. deep. The chief is represented sitting in the conventional cross-legged pose, his left hand holding the Ikula knife, his right on his knee. His ornaments are: the flat cap of the period, a belt ornamented with cowries round his waist, a belt round his loins supporting a sitting-cloth (which, in the carving, is disconnected from it), armlets and wristlets; all these are indicated by carving. Round his neck is an engraved brass ring.

I mentioned above that the statuettes of the three predecessors of this chief are reputed to be by the same artist, but there are certain stylistic features exhibited by the figure under discussion which lead one to think that it was carved by another hand. If this assumption is correct, then the five surviving statues are the work of three distinct craftsmen. No. 1, the carver of Shamba, set the pattern, which was followed closely by No. 2 and No. 3. The work of No. 1 is characterised by an austere severity, almost an angularity, of treatment, which seems common to all styles to which the term "archaic" is applied.

In the case of No. 2, we have the advantage of being able to compare three of his works, but it is clear that, while conforming to precedent, he adopted a freer and more realistic method: the austerity is softened by more generous curves, and

there is an evident attempt to express individual personality. In this respect the figure of Bope Pelenge, whose obvious *embonpoint* has been so appreciatively expressed by the artist, within the conventions set by his predecessor, is, in my opinion, one of the masterpieces of African sculpture.

Artist No. 3, the sculptor of the figure illustrated in Plate M, was inferior to his predecessors in the expression of character. As a work of art, his statue, whether viewed from the front or in profile, is far less impressive. The head is less well-shaped, and, though his "sitter" may have had a "weak" chin, the face lacks character. In two points, it is true, he is more correct than Nos. 1 and 2: he has given a truer proportion to the lower limbs, realising that the splay of the knees of a human body, seated in the cross-legged position, had been underestimated by them. Further, he has spent far more care on the detailed carving of the hands. In spite of this, his work is less harmonious and less effective as a whole than that of the earlier sculptors.

But there is one point which invests this carving with particular interest. When a Bushongo chief had his portrait carved, some emblem, particularly characteristic either of him or of his administration, was sculptured in high relief before the plinth of his effigy. Thus Shamba sits before a *Lela*-board, the board on which the game, known generically as *Mancala*, is played, which was introduced in his reign. Bope Pelenge, a notable worker in iron, is confronted by an anvil. Misha Pelenge Che and Kate Mbula, having no special claim to distinction, are given merely the royal drum, carved with the pattern which they had adopted as their own particular device. Now, in the case of Mikope Mbula, it will be seen, on reference to the illustration, that the personal emblem sculptured in front of him is a human figure. Reference to the monograph on the Bushongo, quoted above, published by the Tervueren Museum, shows that the principal feature of the reign of this chief was a change in social customs. Hitherto marriage between free men and slave women was a social impossibility: but Mikope Mbula became enamoured of a beautiful slave woman and insisted on making her his legal wife. From that time, marriage with slave women became a legalised practice. It seems perfectly clear, therefore, that the figure carved in front of the figure of this chief represents his slave-wife, and commemorates the introduction of a new social practice.

In support of this interpretation, I may call attention to the fact that the story of the introduction of slave-marriage was collected by Mr. Torday more than ten years before the statue of Mikope Mbula was discovered. Further, that at the time of Mr. Torday's expedition, a daughter of this chief, a very aged woman, was still alive, and the events of his reign may, therefore, be regarded as coming almost within the sphere of contemporary history. In any case, it is satisfactory to know that this figure, important for the study both of African art and of African history, is preserved in the Tervueren Museum.

T. A. JOYCE.

Anthropology: Physical.

Keith.

Was the Chancelade Man akin to the Eskimo? By Sir Arthur Keith, M.D., F.R.S.

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In MAN, 1925, 98, the distinguished Professor of Geology in the University of Oxford discusses two matters—the application of mathematical formulæ to the estimation of cranial capacity and the racial characters of the Chancelade skull. Whether this skull has a capacity of 1,530 cc., an estimate given by the formula regarded as the most satisfactory by its authors, Professors Pearson and Dr. Lee, or one of 1,730 cc., as Testut found by direct measurement, has no bearing on the racial nature of the Chancelade man. It is not only among the Eskimo that we find individuals with big brains, there were people living in France long before the time of the Chancelade man who are renowned for their size of brain—that

complex of humanity which is grouped round the Crômagnon type. In size and shape of brain the Chancelade man has more in common with certain individuals of the Crômagnon type than with any known form of Eskimo. Before we set out to seek, as Professor Sollas has done, a living representative of the Chancelade man in Greenland, we must first make certain that this individual has no relation to a type which is much nearer to him in place and in time. I regard Chancelade man as a mere variant of the Crômagnon type; that type is European, or Caucasian if you like; he and the Crômagnon people are of the so-called white race. Professor Sollas, on the other hand, regards the Chancelade man as a Mongol, the particular variant which he has chosen for comparison being the Eskimo.

How are such racial problems to be settled? By a procedure with which I have been familiar for well over half-a-century and Professor Sollas for even a longer space of time. We became, like everyone else, anthropologists as soon as we could distinguish one face from another. Our practice and proficiency in the art of racial discrimination, both for man and beast, increased with our years. As boys we did not need callipers, nor indices, nor any mathematical procedure to assist in telling one breed of dog from another breed; we made our identification as soon as our eyes fell on the beast. We applied the same method when we went out into the world and met the diverse races of mankind. We identified Chinaman, negro, and Indian as they passed us by the mere stroke of the eye. That is just how we identify skulls, after one has become duly familiar with their various racial forms. It is with skulls as with races: certain are so well characterised that the tyro soon learns to recognise them with certainty, there are other racial types which are less well marked, there are all degrees of differentiation; but, fortunately for me, the type in dispute—the Eskimo—is, perhaps, the most sharply characterised form of human skull known to us, particularly the male type, and we all agree about the Chancelade skull being that of a man. The only skulls with which that of an Eskimo can be confounded are those of the Mongolian peoples of the Arctic regions of North-eastern Asia and of North America.

I mention these somewhat elementary matters because, apparently, Professor Sollas believes that a craniologist, when a skull of unknown history is placed in his hands for racial identification, at once sits down and measures its angles and calculates its indices and then sets out to search for skulls possessing similar angles and indices. Craniologists, I am glad to think, have still a trace of humour as well as of common sense left to them; they do not stop strangers in the street and ask their permission to take angles and indices in order to see if they are negro, Mongol or Caucasian; I do not believe that even Professor Sollas does this; a cast of the eye is sufficient for a diagnosis in making a racial identification of a man or of a skull. The anthropologist follows the practice of everyday experience; wherein he differs from the ordinary man is that he becomes conscious of the points which lead him to a diagnosis; he turns a subconscious empirical process into one which is conscious and scientific in that he notes the various points on which a recognition is made, measures them and learns the value which should be attached to them. It is in such a way that the problem of the Chancelade skull has to be approached, and I cannot understand how one, who is familiar with the craniological features of Eskimo in particular and of Mongols in general, can assign this skull to a Mongolian breed of mankind.

Professor Sollas throws the entire onus of mistaking the racial nature of the Chancelade skull on the late Professor Testut. This is somewhat ungallant on the part of one who has advocated so warmly and so emphatically the belief that the ancient hunters of Europe were Eskimos. Professor Sollas forgets that he and I have much better opportunities of knowing the craniology of the Eskimo than was ever possible for Professor Testut in Lyons thirty-six years ago. We have at

our disposal the collections in England, particularly that in the Museum of the Royal College of Surgeons. We have had made accessible to us an excellent and fully documented account of nearly 350 skulls of Eskimo which was published ten years ago in Copenhagen—the *Crania Grænländica*, compiled by Professor Fürst of Lund and Professor Hansen of Copenhagen. These thirty-six years have seen our knowledge of the men who lived in Europe in late palæolithic times trebled in amount and rendered infinitely more exact in quality. It must be remembered, too, that Professor Testut published his paper in those early days when anthropologists believed that races and cultures spread together; had it not been for a resemblance of the Chancelade culture to that of the Eskimo no one would have seen any resemblance of the Chancelade skull to that of an Eskimo. It is truly wonderful how far suggestion can go, especially if a diagnosis is made on such a basis as that used by Professor Testut. The list of diagnostic indices he employed, and which Professor Sollas has cited with approval, would bring the skulls of thousands of genuine Europeans into the Eskimo fold.

How, then, do we know that the Chancelade skull is not that of an Eskimo? In the first place, it has not that remarkable and characteristic combination of characters which centres round the chewing muscles of the Eskimo, and of the bony supports from which and on which these muscles act. The temporal muscles in the Eskimo have extended their origin over and beyond the parietal eminences to an extent unknown in other modern races or in any breed of fossil man; the Chancelade skull shows no such extension. The parts on which the masseter works—the zygomatic arch and the ascending ramus of the mandible—have not the conformation seen in Eskimo skulls, but that found in Crômagnon skulls. The outstanding feature of the Eskimo zygomatic arch is seen at its anterior part—that part which is formed by the malar buttress of the upper jaw. In the skull of the male Eskimo this buttress is larger and higher than in any other race; its size is indicated by the breadth of the face measured between the lower ends of the malo-maxillary sutures: in the Eskimo skull this width may exceed 130 mm.; the mean for twenty male skulls in the Museum of the College of Surgeons is 107 mm.; in the Chancelade skull, as in most male of the Crômagnon people, this diameter measures 100 mm. Professor Sollas cites the great width of the ascending mandibular ramus—43 mm.—as being an Eskimo character, apparently forgetting that even wider rami occur in the Crômagnon people. Then, to explain why the Chancelade man has not the wide projecting jowls of the Eskimo—the bigonial width of the male Eskimo mandible has a mean measurement of 114 mm., whereas in the Chancelade man Testut found it to be only 93 mm.—Professor Sollas resorts to a surgical explanation. My daily vocation has given me some experience of such matters, and I can assure Professor Sollas that his explanation is quite wrong. The ascending ramus of the right side has not suffered from the skull injury, for it is of the same size and form as that of the left. The slight asymmetry on the right side of the Chancelade mandible has a much simpler explanation: it is due to a small error on the part of the restorer, who has failed to give the parts their due place in mending a break in the jaw to the right of the symphysis. Even if this error is eliminated the bigonial width of the jaw will not exceed 100 mm. In Crômagnon mandibles the bigonial width of the jaw is often as great as in that of the Eskimo, but this was not the case in the Chancelade man, in whom there is distinct evidence of a retrogression in jaw development.

In making a racial diagnosis of a skull the conformation of the bony parts of the nose give a more reliable guide than almost any other part. In the Eskimo, as in all branches of the Mongolian stock, the nose—particularly the nasal bones and their supporting structures—have undergone, or are undergoing, a retrograde change. The flatness of the Mongolian face is not due to a forward growth of the

cheeks but to a shrinking backwards of the bony framework of the nose. The Chancelade nasal bones have been broken away, but their roots are in place, and such roots were never seen in the skull of an Eskimo; the interorbital septum, which really forms part of the root of the nose, and the configuration of all the parts round the nasion have no resemblance to the same parts of the Eskimo, but are in every respect European. These parts are exactly as we know them in the Crômagnon people. In no Eskimo skull I have ever seen, and I have examined actual specimens, or faithful drawings of them, to the number of a hundred, have I found the sharp bony ledge and jib-like nasal spine which form a sill to the nasal opening of the Chancelade skull. Such a conformation is sufficient to tell anyone, familiar with recent advances in human craniology, that the Chancelade man, whatever he may have been, was not an Eskimo. A further argument is found just below the nasal sill in that part of the jaw which carries the upper incisor teeth: in Eskimos, as in all Mongolian peoples, there is a greater or less degree of sub-nasal prognathism: in the Chancelade man there is a total absence of prognathism. The Chancelade skull has a long nose—61 mm.—and its width was moderate—26 mm. Noses with such dimensions occur among the Eskimo: so do they also among Europeans, particularly among the men of certain parts of France. The late Anatole France, if one may judge from his photograph, had a nose of such dimensions, and he had no more resemblance to the Eskimo than had the Chancelade man. One of the skulls recently found at Solutre, in a stratum of Aurignacian age, has a nose of similar dimensions.

I should try the patience of readers were I to subject all the other parts of the Chancelade skull to a similar process of racial analysis; suffice it to say that a development of supraorbital ridges, similar in size and form to those of the Chancelade skull, does not occur in Eskimo skulls. The Chancelade skull is high-roofed; so is it often in skulls of the Crômagnon people and also in the Eskimo, but the form of the hinder region of the vault of the Chancelade skull, in its contour and declination, has never been seen in an Eskimo skull. The Chancelade skull shows a sagittal ridging on its vault, but that is a craniological feature of many primitive races; it can be seen occasionally in modern English heads.

I can hardly hope to convince Professor Sollas that the Chancelade man is no more an Eskimo than either of us. If he is not, then the lofty pyramid which Professor Sollas has reared comes tumbling down. Under such circumstances, it is too much to expect him to be impartial. But I do hope that I may convince your readers that the right way to identify the racial nature of any given skull is to apply to the task the same method as Linnaeus used for the discrimination of species—the recognition of marks which are characteristic of race. Absolute measurements are of great assistance, but the use of indices and angles by themselves, as in the case now in dispute, can and do give most misleading results. One other qualification is needed for the equipment of the craniologist—a complete and intimate knowledge of the skulls of all races of mankind. ARTHUR KEITH.

Anthropology: Physical.

Pycraft.

On the Recognition of several Species of Post-Mousterian Man: and the Need for superseding the Frankfort Base-Line. By W. P. 117
Pycraft. *Continued from MAN, 1925, 105.*

And now as to my methods of analysis. These are based primarily on the use of superimposed contours: a method I adopted in my Report on Skulls from New Guinea, already referred to. They have been used, and with striking effect, by Sir Arthur Keith and Professor Sollas. Who was the first to use this method of comparison I do not know, but its value, I think, has been amply demonstrated.

Next—and I maintain that this is an important departure—I use a Base-line of my own—the Auriculo-nasion Line, which, as its name implies, passes from the nasion backwards through the centre of the auricular meatus. This line practically cuts off the whole of the cranium from the face. All other anthropologists, save Sir Arthur Keith, use the “Frankfort Line,” than which it would be difficult to find a more useless, or misleading base. Sir Arthur Keith’s Line passes backwards from the external angular-process of the frontal through the asterion. I must confess my inability to see its precise value.

The great merit of the Frankfort-line, so say its champions, is that it places the skull in the position it assumes in life when the late owner of the skull was looking straight ahead of him. The merit of mine, judged by this standard, is that it places the skull in the same position as when that owner was thinking!

But we ought to have more serious reasons for our methods than these. The meato-nasion, or auriculo-nasion line, apart from the fact that it can be used for skulls in which the facial portion is wanting—and this is often the case—has one other advantage possessed by no other. After the facial angle has been taken, one has but to add 20 to the number given by that angle to obtain the Alveolar-Index as obtained on the calculation devised by Flower.

Where the resultant indices differ, the error will be found to lie with the result obtained otherwise by calculation, which Flower himself admitted was but an approximation to the truth.

The reason for the failure of Flower’s method is easily demonstrable. It fails because its fixed point is the basion, which is extremely variable. It may be no more than 5 mm. below the auricular aperture, it may be 20 mm. It may be in advance of this aperture, or behind it. And this shifting may make a very material difference in the relative lengths of the basi-prosthion and basi-nasion lines.

But let me press this point a little further. I want to show that there is an intimate relationship between the facial angle, as derived from my base-line,—but no other—and the alveolar index, which is governed, not as Huxley supposed, by movements of the anterior base of the cranium, but by (a) the length of the meato-nasion line, (b) the alveolar length, and (c) the basi-alveolar distance, this last governing the size of the respiratory space behind the posterior nares.

It will be found that when skulls having the same, or approximately the same, meato-nasion length are compared, the difference in their facial angles can be accounted for, by comparing the differences between the combined alveolar length, and the basi-alveolar distance in each skull in the series under examination. The two following examples should make this point clear. I have selected these at random, the better to establish my case. The first two are skulls of a New Caledonian and a Burman:—

New Caledonian	- - -	M-n. 93 F.a. 89°; A.l. 1.60; Ba.-d. 47.
Burman	- - -	M-n. 93 F.a. 77°; A.l. 1.55; Ba.-d. 39.

The meato-nasion lengths (M-n) it will be noted, are equal, while the difference in the facial angle is as high as 12°. This difference can be accounted for by the difference between the sum of the alveolar lengths, and the basi-alveolar distances (A.—l : and B.-d). Thus in the skull with the lower facial angle the alveolar length has decreased 5 mm. and the basi-alveolar distance 8 mm. = 13 mm.

The skulls of a Papuan and a Tasmanian similarly compared show like relationship:—

Papuan	- - -	M-n. 90 F.a. 88°; A.l. 66; Ba.-d. 44.
Tasmanian	- - -	M-n. 92 F.a. 82°; A.l. 58; Ba.-d. 45.

The meato-nasion lengths in these two cases, it will be noted, are slightly different. Between the facial angles there is a difference of 6, the lesser angle being

due to the shorter alveolar border, which is less than that of the Papuan by 8 mm. The difference between the sum of the alveolar and basi-alveolar lengths in the two skulls is 7, thus again accounting for the difference in their facial angles. This striking correspondence between the length of the jaw, the basi-alveolar distance, and the facial angle, is exemplified wherever two skulls having the same, or approximately the same, meato-length are compared, which sufficiently demonstrates my contention that the facial-angle is directly dependant on the length of the jaw, and not on the movements of the basi-cranial axis. The downward thrust of the anterior portion of the basi-cranial axis referred to by Huxley, was brought about by the great increase in the volume of the brain. It does not, apparently, affect the movement of the face now under discussion.

A further illustration of the soundness of this theory is furnished by the fact that, given the angle, alveolar-length, and basi-alveolar distance of one skull, the facial angles of any others, of which the sum of the alveolar length, and basi-alveolar distance is known, can be found by a simple sum in proportion, provided that skulls of nearly the same meato-nasion lengths are compared. The skulls of the Papuan and Tasmanian just referred to afford an example of this test :—

$$110 : 88^\circ :: 103 : \text{Ans. } 82^\circ.$$

The total length of the skull is not a factor in these calculations.

To return now to the Gnathic-Index. Since the figures derived from the facial-angle agree so closely with those obtained from Flower's method of calculating this index, the standard of gnathism need not be changed; but as the continued use of the term "prognathism" is open to objection, I propose to substitute the term "Megalognathous." The Gnathic Index may then be formulated as follows :—

Angle less than 78° Orthognathous.

Angle between 78° and 83° Mesognathous.

Angle above 83° Megalognathous. (Prognathous.)

Finally, I venture to express the opinion that the Base-line now proposed, affords what no other Base-line will afford, a reliable and uniform standard of comparison between the skulls of different types, since it has reduced the sources of error to a minimum.

In the event of my proposal for the recognition of an indefinite number of species of the Genus *Homo* being adopted, I would urge that when a new species, or sub-species, is described, that description should be accompanied by a contour-tracing of the *Norma lateralis* of the skull, at least, and where possible of the *Norma verticalis* as well. Workers in distant parts of the world will then be able to take immediate advantage of the detailed description. It is indeed imperative that this course should be followed, since no description, in itself, can enable one adequately to visualize the precise form of its contours.

To judge by results, anthropologists have no great reason to be over-proud of their methods of work: and I want to appeal to those engaged in craniological work to give this matter of the Frankfort-line their serious reconsideration. I contend that it has not a single redeeming feature, and should henceforth be discarded.

When this paper was read, at the Southampton Meeting of the British Association, it was urged that though the Frankfort Line was admittedly faulty, yet it must be retained, because, forsooth, it had been so long in use. Could any more fatuous reason be given for the retention of an avowedly faulty means of attaining scientific results? Is it not the very negation of the methods of the man of science to employ, and employ deliberately, faulty methods of research? If we, as anthropologists, thus shamelessly abandon the very essentials of scientific investigation—accuracy in our methods—we shall inevitably bring ourselves, and our work, into

contempt. It seems incredible that there should be found any among us so lacking in imagination, so wedded to convention and tradition, as to go on using a standard of comparison whose only merit is that it has been in use for long years. Rather should we follow the Pauline injunction "Prove all things: hold fast that which is good."

Furthermore, this precious line is absolutely useless save in skulls wherein the face is intact, at least as far as the lower border of the orbit.

In how many fossil skulls is this the case, and in how many of the thousands of skulls taken from ancient graves? With the line that I propose all these skulls are made available for comparison with modern skulls.

I append two tracings of the same skull, the one on the Frankfort, the other on the meato-nasion line. In the one it will be seen that half the face is above and half below the line.

In the other the whole face is below the line, the whole cranium above it. They speak for themselves!

Apart from the advantages already pointed out, which are to be gained by the Meato-nasion Base-line, there is the further merit that it

THE SKULL OF A TASMANIAN PLACED ON THE MEATO-NASION AND FRANKFORT BASE-LINE.

emphasises the salient features of the skull in a way impossible when the Frankfort line is used. My tracings will surely make this point clear.

Finally, we are told that, be this as it may, we cannot now adopt another line, because so many thousands of skulls have been displayed on this "conventional" line! This is a feeble enough argument. But it is worse than this, it is a confession of the lack of mental alertness, which is, indeed glaringly apparent in our persistent use of "Average Indices," and our innumerable schemes of classification, only some of which present more than an approximation of the truth! Surely, as, and when, we need to compare any published figure of a skull displayed on this ridiculous Base-line we can easily, at the cost of a piece of tracing-paper, transfer it to the Meato-nasion Base-line.

This deplorable contempt for accuracy of method, born apparently of desire to save trouble, and defended, too, at a Meeting of the British Association for the *advancement* of Science, is reflected in the use of the Orbital Index, than which a more ridiculous it would be hard to find.

Finally, it does not seem to have occurred to those who put their faith in "Average Indices" that these cannot, by any possibility, afford us any index whatever of racial affinities, which is supposed to be the sole reason for their use. What they do afford, if they afford anything at all, is an indication of the results of "mongrelisation." They yield but shadows of reality, which we are asked to accept as concrete facts. Is it not time that we faced the situation and started afresh?

The analysis of any given series of crania devised to discover their genetic relationships, cannot be made with the ponderous methods of the biometrician. They will not even give us a trustworthy indication of the results of interbreeding between alien races—the "mongrelisation" to which I have referred.

An admirable example of the futility of such methods is furnished in "A Study of the Negro Skull with Special Reference to the Congo and Gaboon Crania," by the

late Dr. R. Crewdson Benington; and edited by Professor Karl Pearson (*Biometrika*. Vol. VIII, 1912).

The material for this "Study" was provided in part by Congo crania in the Royal College of Surgeons, and in part by the Gaboon crania collected by Du Chaillu, and Angoni skulls, presented by Sir Harry Johnston, in the British Museum of Natural History.

I have seen none of these crania save those under my charge in the British Museum. But my examination of the Du Chaillu collection suffices to show the absolute uselessness of the whole investigation. And this because the skulls are treated as though they were all of a pure negro race, albeit displaying a remarkably wide variability. On this assumption they were sorted out into large, male, and small, female skulls, and forthwith run through the biometrical mill. The result yielded some very astonishing conclusions! . . . They would.

As a matter of fact these Gaboon crania so far from representing a pure race, displaying a wide range of "variability," prove instead to be of very mixed descent. Negro, Negrillo, Bushman, Bantu, and Hamite, in varying combinations, have gone to the making of these skulls. The Bushmen and Negrillo hybrids apparently furnished Dr. Benington with his female, and the Negro-Bantu-Hamite with his male skulls!

The Editor draws a distinction between the methods of the Craniologist and the Biometrician. This is undoubtedly justified. Again, I ask, of what use are the "Coefficients of Racial Likeness" expressed in imposing formulae when applied after this fashion?

The biometrical method of investigation will, no doubt, yield useful results when applied to craniology, if only those who use it will take the trouble to analyse carefully the nature of the material to be subjected to this treatment before getting to work.

At the risk of a certain amount of repetition I would briefly review the methods which it seems to me must form the basis of our work in future, in the matter of the analysis of crania for the purpose of discovering their genetic affinities.

As I have already indicated we must build upon the foundations furnished, in the first place by skulls like those of *Eoanthropus*, Mousterian man, the Aurignacian and Magdalenian types, the Boskop, Negro, Pre-Dravidian, Tasmanian and Polynesian types, and so on. These and others yet to be defined are to form our standards of comparison. One or other of these will have to be taken into consideration in our endeavours to distinguish specific forms of the Genus *Homo*. They will afford us positive standards of comparison, enabling us to detect the character and degree of "mongrelisation," or miscegenation, which has taken place in any given skull.

The superimposed contours of the *Norma lateralis* will prove the best guide for the initial stages of the investigation. The clue thus afforded must be followed up by careful comparisons between the various *normæ* as well as in regard to other structural characters, to be found not only in the cranium, but also in other parts of the skeleton. Nor may we neglect evidence afforded by dissection which must be used in addition to the more superficial characters hitherto alone taken into serious consideration.

DIAGNOSES OF TWO NEW SPECIES.

PAPUAN. *Homo papuensis*.

Skull with glabella swollen: Tasmanoid: supraciliary ridges large, but ill-defined: nasals long, and narrow: expanding distally; dental arch long, palate deep: frontal region with a distinct median ridge: parietal scaphoid: parietal eminences large.

Type skull: Coll. Mus. Brit. (Nat. His.), No. 1914, 5.10.4 (Utakwa River:—L. 191, B. 137 (I. 72), Bb. 127 (I. 66), Bp. 98, Bn. 96 (I. 102), Nw. 22, Nh. 47 (I. 46), Np. 65, Bz. w. 127 (I. 51),

Oh. 36, Ow. 44. Dolichocephalic. Tapeinocephalic. Mesognathous. Leptorrhine Chamaeprosopic. Phænozygous. Fig. 6, Pl. II, Rep. Human Crania, Collected: New Guinea. 1911. **HIMALAYAN.** *Homo himalayensis.*

Skull with glabella and supraciliary ridges well marked: forehead with a slight median ridge: Metopic: Parietal eminences moderately developed: Inion sharply defined: orbits large: inter-orbital septum enormously wide: nasals moderately long, and concave: malar prominent: palate very high: alveolar border horse-shoe shaped: mastoid, and pterygoid conspicuously large.

Type skull: Coll. Brit. Mus. (Nat. Hist.) Reg. No. 58, 6.24.19I. I'nu Bhotia: Hodgson Coll. L. 186, B. 142 (I. 76), Bb. 135 (I. 72), Bp. 102, Bn. 101 (I. 100), Fa. 76°, N.W. 28, Nh. 50 (I. 56), Np. 75, Bz. 137 (I. 57), Intersthenotic 111 (I. 81), Oh. 35, O.W. 42. Mesaticephalic. Metriocephalic. Mesognathous. Platyrhine. Leptoprosopic. Phænozygous.

In my Boskop Report (*J.R.A.I.*, Vol. LV.) I endeavoured to keep my diagnoses as short as possible. This was a mistake. I would now add the following particulars and amendments:—

TASMANIAN. *Homo Tasmanensis.*

Skull Metopic: Megadont: N.p. 62, Bz. I. 35 (I. 46), O.h. 27, O.w. 37, F.a. 92°.

AUSTRALIAN. *Homo antiquus.*

Add N.p. 69, Bz. 138 (I. 50) O.h. 32. O.w. 40. For B.n. 198 read 108.

POLYNESIAN. *Homo sandvicensis.*

After "well developed" add "glabella (line 1). Alveolar Index should read 99." After "occipital protuberance" delete "glabella well developed," and add: "palate long and narrow." Add F.a. 74°, O.h. 39, O.w. 39.

CORRECTION.—In Pl. L for Fig. 1—Negro read Fig. 1—Negrito; for Fig. 3—Negrito read Fig. 3—Negro.

W. P. PYCRAFT.

Magic: Colour Symbolism.

Bonser.

The Significance of Colour in Ancient and Mediæval Magic: 118 with some modern Comparisons. By Wilfrid Bonser.

Colour is a significant factor in folk-medicine. It may be important that (a) a thing should be all of one colour, (b) of a specific colour, and (c) piebald or variegated, especially in the case of clothing. This last is usually the case when the rites have become complicated. The same principles seem to hold in most races.

(a) *One-colour Magic.*—Two examples of one-colour magic occur in Anglo-Saxon leechdoms. "For flying venom . . . on a Friday churn butter which has been milked from a neat or hind all of one colour": and again, "The woman who cannot bring her child to maturity, let her take the milk of a cow of one colour in her hand, and sip it up with her mouth."†

Pliny says of the agate, "the stone that is of an uniform colour, renders athletes invincible."‡

Dioskorides gives a cure to be effected by hanging round the neck of the sufferer two stones, to be found in the maw of a swallow, "unum quidem colore varium. alterum purum et unicolorum."§

Similarly, in an Irish charm against shingles, nine drops of blood from the tail of a male cat, which is all black and has no whiteness on it, are a necessary ingredient.||

The following legend of Newington church, near Chatham, is also relevant. The Evil One deterred the building of it; and on the first night after the bell was hung it was removed and thrown down a neighbouring well. The villagers had resort to a witch, who said it must be drawn out by twelve jet black oxen. When

* "Leechbook of Bald," Book 1, 45, v.

† "Lacnunga," 104.

‡ Pliny: "Nat. Hist." xxxvii, 54.

§ Dioskorides, "Parabilium" (1598), Book 1, 7.

|| R. Marlay Blake: "Folk-lore, with some Account of the ancient Gaelic Leeches" (*Journal of County Louth Archaeological Society*, iv, 3, Dec. 1917).

the bell neared the top, it suddenly fell to the bottom again. It was then found that one of the oxen had a few white hairs in the cleft of one of its front feet. Another all-black ox was then obtained, and the bell was successfully raised and fixed, nor did the Evil One again interfere.*

(b) *Specific Colours.*—The difficulty of identifying the specific colours of the ancients is due to the uncertainty as to the meaning of the words used. In contrast with the Greeks, the Anglo-Saxons had words for most colours. Nine colours occur together in the famous charm† in which the nine specially magical herbs “avail against nine spirits of evil, against nine venoms, and against nine “ winged onsets, against the red venom, against the foul venom, against the white “ venom, against the purple venom, against the yellow venom, against the green “ venom, against the livid venom, against the blue venom, against the brown “ venom, against the crimson venom.”

The individual colours were often associated for magical purposes with those things which obviousness suggested: *e.g.*, red with blood, yellow with jaundice and the liver, the spotted leaves of lungwort with diseases of the lungs, and so on.

Plants with red blossoms, berries or roots were used for cases of haemorrhage. Examples are the red seed of paeony and the red root of astragalus.‡ There is one instance of this—in Sextus Placitus—which does not quite conform to type. Here the fruit of the morbeam, or mulberry, is to be taken (with the thumb and ring finger of one left hand) *before* it has become red.§ The passage occurs in the Anglo-Saxon version, but not in the original Latin. As the idea appears to be to stop red (*i.e.* blood) from coming, so something which might have become red is used before it turns red; and it is also prevented from becoming red. This connection persisted until modern times, for the *unripe* fruit of the mulberry is prescribed by Colbourne for “spitting of blood.”||

It is not always red flowers that are prescribed in cases of haemorrhage. In the whole of the *Herbarium* of Apuleius, I can only find one red—and that not a blood-red—flower used for this purpose. This is betony, which is to be used to stop blood both from the nose and from the mouth.¶

The use of a red fillet for tying herbs round the head in cases of headache** is less obvious, but is perhaps explainable in that red is the colour for expelling demons. It is also prescribed, in the case of a lunatic, that clove-wort should be tied round the man’s neck with a red thread: if done in April or early October, when the moon is on the wane, “soon will he be healed.”††

Pliny says the Magi attributed many very wonderful properties to the anemone—a plant with a scarlet flower. As a remedy for tertian and quartan fevers “the flower “ must be wrapped up in a red cloth and kept in the shade, in order to be attached “ to the person when wanted.”‡‡

Red is still considered obnoxious to evil spirits: *e.g.*, the red berries of the rowan are used, as in mediaeval times, to keep away witches.

Red flannel was worn round the neck for sore throat and whooping cough the flannel gave the necessary warmth, but the colour was considered important

* Told me by Mr. Henry Davis, a native of Chatham. Though the church is mediaeval, the legend is still current.

† “Lacnunga,” 45.

‡ Pliny: “Nat. Hist.,” xxvi, 82.

§ Sextus Placitus: “Medicina de quadrupedibus,” I. 5.

|| Colbourne: “Plain English Dispensatory,” 1753, p. 70.

¶ Apuleius: I, 7 and 13.

** “Leechbook of Bald,” Book 3, 1.

†† Apuleius, “Herbarium,” X. 2.

‡‡ Pliny, “Nat. Hist.,” xxi, 94.

for magical reasons. This is evidently a sympathetic connection between the redness produced by the inflammation and the redness of the material applied.

In Assyrian medicine there is an incantation for a sick (*i.e.*, bloodshot) eye. "Ritual for this: red wool, white wool, separately shalt thou spin: . . . the thread of red wool shalt thou tie on his eye which is sick, the thread of white wool shalt thou tie on his eye which is whole, and he shall recover."^{*} Many other Assyrian examples of the magic of red things could be given.

The most celebrated case of the sympathetic use of red in the Middle Ages is John of Gaddesden's cure of Edward II.'s son when suffering from small-pox. "Feci 'omnia circa lectum esse rubea,'" he says, "et est bona cura, et curavi eum in 'sequenti sine vestigiis variolarum.'"† John of Gaddesden was, however, unaware of the scientific explanation of the use of red light in the treatment of small-pox, although he may have known by experience of its utility.

The connection between the colour red and small-pox has survived in the practice of wearing a red ribbon round the arm after vaccination.

The symbolic use of red paint in connection with small-pox is found in India at the present day. The chief deity of the Marias, Matadevi, is small-pox. She is represented by "a stone set up by the side of the road and dappled with red paint."[‡]

In Southern India, when an outbreak of small-pox threatens a village, the inhabitants make a horse of clay, painted white and with scarlet trappings, which they take in procession with the image of the goddess, who is named Sowaramma, the riding goddess. They leave the horse under a tree outside the village, in order that the goddess may ride away on it. If no horse is provided, she will remain in the village. All offerings to the goddess are scarlet in colour: cooked rice, for instance, when offered is dyed red with saffron and chunam.

The following black magic is still practised in Southern India: it is also a remedy for, and to ward off, diseases. A small pot, full of scarlet-coloured water (again produced by a mixture of saffron and chunam), is thrown down and broken so that the water is spilt, where three roads meet. Coins are also put in, and whoso picks them up is supposed to have the disease transferred to him.

Red water (with rice in it) is used at all festivals. It is waved in front of a child on its birthday. At the *holi* festival in honour of Kali, god of love, which is held a fortnight before the beginning of the spring, rose-coloured water, called *wasanta*, ("spring"), is squirted over everybody.[§]

"To dream of eating anything red brings disease" runs an Aino proverb.^{||}

Verses from the Koran, written on paper and enclosed in a small leather case, are worn in Egypt at the present day as charms, both by men and animals. For women and children the case may be of any colour, but for the important creatures—the men and the donkeys—the colour is always red.[¶]

As regards yellow, flowers of this colour were given for jaundice and disorders of the liver: yellow-wort (*chlora perfoliata*)^{**} and flea bane are †† examples.

Lady Wilde says that "homoeopathic adepts among the Irish doctors always employ yellow medicines for the jaundice, as saffron, turmeric, sulphur, or even yellow soap."^{††}

* Thompson, "Assyrian Medical Texts," 15 (*Proc. Royal Soc. of Medicine*, vol. 17, pt. 4. Feb. 1924).

† John of Gaddesden: "Rosa Anglica," fol. 51, recto, col. 2.

‡ Holdich: "The Mardian Hills and the Lower Indravati in the Bustar Dependency" (*Proc. Royal Geogr. Soc.*, N.S. 1, 1879), p. 380.

§ I am indebted for these Indian examples to Mr. M. H. Krishniengar, of Mysore University.

|| Chamberlain: "Aino Folk-Tales" (Folk-Lore Society), 1888, p. 57.

¶ I am indebted for this to Miss M. A. Muriaay.

** Apuleius: "Herbarium," xxxv, 1. †† Dioskorides: Book 3, 126.

†† Lady Wilde: "Ancient Cures, Charms and Usages of Ireland," 1890, p. 19.

Frazer mentions a ceremony among the ancient Hindoos for banishing jaundice into three yellow birds—the parrot, the thrush and the yellow wagtail—by means of yellow porridge.*

Pliny says: “I find it stated that in the most ancient time, yellow was held in the highest esteem, but was reserved exclusively for the nuptial veils† of females, for which reason it is perhaps that we do not find it included among the principal colours, these being used in common by males and females: indeed it is the circumstance of their being used by both sexes that gives them their rank as principal colours.”‡

Colours, especially in a fixed order, were of great importance in iatro-chemistry. The pseudo Basil Valentine (16-17c.) holds that antimony contains all the colours, namely “black, white, red, green, blue, yellow, and more other mixt colours than can be believed, all which may be separated apart, and known particularly, and singularly applied to use.” It is, therefore, a panacea for all ailments. “But as all the colours of all metals and precious stones are clearly found in antimony; so also all the powers and virtues of medicine are no less shewed in it, than the colours aforesaid.”§

In ancient India, plants of certain colours were believed to have magical qualities. “The plants that are brown,” runs a passage in the Atharva-veda, “and those that are white, the red ones and the *speckled* ones [of which more anon], the sable, and the black plants, all these do we invoke.”||

Similarly, the sacred magic colours of the Zuñi Indians of New Mexico are “yellow, blue, red, white, *spotted*, and black.” They have names for other colours, but only these have magic power.¶

(c) *Piebald*.—Finally, with regard to the employment of the variegated, speckled, or piebald in connection with magic, the more complicated the magic, usually the more this obtains.

Perhaps the example from Basil Valentine, just quoted, applies to this section as well as to that of specific colours, especially as it is late and complicated.

Turning to Egypt in the days when she was the home of “mysteries,” one finds that Isis, in the Demotic Magical Papyrus, is called “mistress of magic, the great sorcerer of all the gods”**. If my thesis holds, one would, therefore, expect to find her attired accordingly. Plutarch says: “Now as to the robes: those of Isis [are] variegated in their dyes . . . while the [robe] of Osiris has neither shade nor variegation.”†† This is remarkable since, in representations, the gods of ancient Egypt are attired in self-coloured garments, without any pattern.

Another late Egyptian charm is as follows: “You take a band of linen of sixteen threads, four of white, four of green, four of blue, four of red, and make them into one band . . . and you bind it to the body of the boy who has the vessel, and it will work magic quickly.”‡‡

Lucan tells us that before the battle of Pharsalia, Sextus Pompeius consulted the witch Erichtho as to the event of the ensuing fight. She works magic spells for him, but first arrays herself in a dress of many colours.|||

The Pied Piper of Hamelin performs his magic in a parti-coloured robe.

* Frazer: “Magic Art,” vol. 1, p. 79.

† Flamineum (=orange-yellow).).

‡ Pliny: “Nat. Hist.” xxi, 22.

§ Basil Valentine: his “Triumphant Chariot of Antimony,” London, 1678, pp. 59-60.

|| Atharva-veda: viii, 7—“Hymn to all Magic and Medicinal Plants.”

¶ Culm: “Magic of Colour” (*Brooklyn Museum Quarterly* for April, 1925), p. 102.

** “Demotic Magical Papyrus,” transl. Griffith and Thompson, col. ix (19).

†† Plutarch: “De Iside et Osiride,” lxxvii, 1.

‡‡ “Demotic Magical Papyrus,” p. 39, col. iv (33).

||| Lucan: “Pharsalia,” vi, 653 *et seq.*

Two examples from the Bible are suggestive: one is Joseph's "coat of many colours,"* which was taken from him by his brethren as being too precious to be endangered. The other is the dress of the High Priest. The ephod "was made of richly variegated stuff of four colours." The breastplate was to be in part of the same material as the ephod, and was to be "of gold, of blue, and of purple, and of scarlet, and of fine twisted linen";† it also bore the twelve different coloured stones on its front face. The breastplate is especially important in this connection since it was used for purposes of divination.

The dragon-god among the Aino appears as a man "dressed altogether in speckled raiment."‡

Piebald horses are often connected with workers of magic. A man on a piebald horse can always give an infallible remedy for whooping-cough.§ Marko, the "King Arthur" of Serbian legend, possessed a magic steed named Sharatz ("piebald"), which was endowed with extraordinary powers. Rustem's Raksch was of a saffron colour, spotted with red. In Japan "three-coloured cats are more powerful in magic than others."||

To return to medicine in its most degenerate period, Marcellus Empiricus prescribes in one case the use of a silver needle with threads of nine different colours.¶

The magic of the Finns is full of the use of variegated things—usually stones.** The Sampo, for instance, has a variegated lid, and is made in a smithy erected beside a "stone all streaked with colours."†† The "speckled stones" used by the Tuatha De Danaan of ancient Ireland may also be mentioned in this connection.

WILFRID BONSER.

REVIEWS.

Ægean: Archæology.

Forsdyke.

Catalogue of the Greek and Etruscan Vases in the British Museum. Vol. I, Part I: *Prehistoric Ægean Pottery.* By E. J. Forsdyke. London: British Museum, 1925. Pp. xliv + 228 with 309 figures and xvi plates. **119**

Outside Greece there is no collection of prehistoric Ægean pottery at once so representative and so extensive as that in the British Museum.

Incidentally it includes the first Mycenæan vases known to science—the pottery from Rhodes excavated several years before Dr. Schliemann began work at Mycenæ. The whole series is now published and described in a masterly manner. Of course a good deal had previously been figured in isolated articles. Still there is much that is new; we may instance the large group of complete vessels from Yortan in Mysia which includes several striking forms. Again the fragments A 700 from Knossos come from "Ephyraean goblets," a type that has hitherto been regarded as peculiarly Peloponnesian. And old friends, too, gain a new meaning when seen in their proper context with the aid of Mr. Forsdyke's illuminating commentary. It was a particularly happy inspiration to place the fragments from Tell-el-Amarna side by side with complete vases of the same type from Ægean sites; for these sherds from the city of Akhenaten are of supreme importance for the chronology of decorative art in Greece. A catalogue such as this must of itself at once become a standard book of reference.

* According to the usual translation. The original Hebrew signifies "patches."

† Exodus: xxviii, 15.

‡ Chamberlain: "Aino Folk-Tales" (Folk-Lore Society), 1888, p. 13.

§ Whately's "Remains." The cure of whooping-cough is also often connected with the colour scarlet.

¶ N. W. Thomas: *Folk-Lore*, 12, 1901, p. 70.

|| Marcellus: "De medicamentis," cap. 29.

** Cf. "Loitsurunoja," § 2a; 10a; 10b; 226a; 232b, etc.

|| "Kalevala," Rune 10.

But Mr. Forsdyke has given us very much more than a mere catalogue. His introduction is the most authoritative account of the development of the several ceramic groups of the Ægean area and their relations with one another and with remoter provinces that we have ever read. This means that it must rank as a first class contribution to European prehistory; for the Ægean cultures are primarily delimited by their pottery.

Chronology is treated with the fullness it deserves and one fresh date is won for science. By the (negative) evidence from Egypt our author can fix at about 1250 B.C. the beginning of that Late Mycenaean B style which Dr. Mackenzie first identified fifteen years ago and brought into connection with the Achæans. At the same time this style itself is precisely defined in a quite novel way by an intensive analysis of the evolution of the Mycenaean lily motive. Its lower limit is less sharp; for though Forsdyke distinguishes it from Sub-Mycenaean wares, such as those from Assarlik and Vrokastro, a certain overlapping between the two groups must be admitted. In every respect this volume is indispensable to all concerned with the archæology of Europe or the Near East. It represents the ripe fruit of an exhaustive study of all the ceramic remains available in Britain and a thorough mastery of the voluminous literature. There is only one point which I would challenge. Had Mr. Forsdyke had the opportunity of visiting Greece and familiarising himself with the products of the excavation in their totality, he would hardly have queried the Middle Helladic date of the Minyan ware from Piperis (p. xxv, note 2); there we meet this fabric in use as domestic ware; its shapes only seem "unusual" because forms imported into other regions for funerary purposes are alone taken as normal; the "Minoan types" which "may" have influenced them" go back to M.M.I. A truer appreciation of this group from Central Greece would have enabled our author to assign an exact origin to the "Helladic feature" which he admits as "probable" in the Late Mycenaean (chiefly "Achæan") class of open bowls (p. 174).

We hope on the whole that Forsdyke's carefully chosen terminology will be regularly followed in the future. We are glad that he has had the courage to assimilate the Helladic periods to the Minoan, eliminating the discrepancies that make the division of Wace and Blegen so confusing. The revival of the term Mycenaean to denote the Minoan or Minoanising fabrics from the Mainland is also to be commended as avoiding the preconceptions inherent in the term Late Helladic; Late Helladic I, II and III should in future be used only of the obviously native wares contemporary with what Forsdyke happily calls Early, Middle and Late Mycenaean. At the same time it must be borne in mind that, as a period, Early Mycenaean would overlap a little with M.M. III.

But we abandon with reluctance long established international names like "matt painted" and "sauce boat." And "parallel arcs" can hardly be used to describe what, at least in Cartesian space, are known as "concentric arcs." Nor may "Bronze Age" be admitted as a synonym for Copper Age in referring to Central Europe (p. xx); the deplorably loose use of the former expression to denote the early age of metals in the Near East can no longer be rectified, but need not be further extended.

V. G. C.

Mesopotamia: Archæology.

Delaporte.

Mesopotamia: the Babylonian and Assyrian Civilisation. By Professor L. Delaporte. Translated by V. Gordon Childe. London: Kegan Paul, Trench, Trübner & Co., Ltd. "History of Civilisation" Series. 1925. 371 pp., 59 illus. 1 map. Price 16s. net.

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French Assyriologists have been engaged since the War in publishing some very useful *œuvres de vulgarisation*. Professor Delaporte followed the surveys of

M. Contenau and l'Abbé C. F. Jean with the present work, which has now been translated into English by Mr. Gordon Childe. The aim of the writer has been to give a comprehensive account of the civilisation by classifying the known facts under detailed headings. The first part opens with a summary account of the history of Babylonia, and proceeds to deal with "The State and the Family," "Legislation," "Economic Organisation," and then passes to "Religion," "The Arts," "Letters" and "Science." The same sequence is observed in the second part, which is concerned with Assyria. The book is throughout marked by sound scholarship and a careful avoidance of theorising and imaginative reconstruction, a rare merit in the case of this subject, which has been overburdened by the wildest kind of speculation both in matters of detail and in general conceptions.

Babylonia and Assyria owe their singular importance for the historian to the fact that they are the only countries, apart from Egypt, in which the course of history during the Chalcolithic and Copper Ages can be certainly traced. The material remains of the civilisation are, unfortunately, always in bad condition, and rarely reach the level of artistic achievement found in the products of Egypt and of the Ægean area. On the other hand, the plethora of documents affords a better basis for the understanding of the political organisation, the magical beliefs, and the intellectual achievements of the Babylonians and Assyrians than is possible in any other case. The story which Professor Delaporte has to deal with—very summarily, from the nature of the case—is that of lands where the most diverse racial elements, Sumerians, Elamites, Kassites, Akkadians, Amorites, Chaldaeans, Aramaeans (and in the case of Assyria, Subaræans should be added) mingled in an inextricable confusion. Yet Babylonian civilisation remained, when Babylon fell before Cyrus in 539 B.C., essentially the same as it is known to us over two thousand years earlier. There is development and change, but it is slow and not so considerable as the historical circumstances might lead us to expect. Egypt, Babylonia, Assyria each afford conditions totally unlike: if theories of the course of history during the Copper Age were always tested by reference to what happened under remotely similar conditions in these lands, would it be possible to approximate to some norm by which such theories might be judged?

It is hardly necessary to state that it would be possible to question some of Professor Delaporte's statements, to deny some of his conclusions, to interpret some evidence differently. This is inevitable in so young a branch of science as Assyriology. The chronology before 1750 B.C. is still a matter of dispute; the trustworthiness of certain texts has been called in question. But in general the reader may turn with confidence to this handy summary. SIDNEY SMITH.

CORRESPONDENCE.

Psycho-Analysis.

Jones.

Psycho-Analysts and Anthropology.

To the Editor of MAN.

SIR,—It was gratifying to learn from the last number of MAN, 1925, 113, that the wish expressed by the present writer for a closer co-operation between anthropologists and psycho-analysts has already met with some response. Mr. Hocart has issued an invitation to psycho-analysts to contribute their share in the solution of a problem which he has dealt with from the point of view of historical development. The invitation, it is true, was somewhat circuitously addressed, in a paper published in the first number of a new periodical called the *Ceylon Journal of Science*, but it is none the less cordially accepted and as soon as I can procure a copy of this journal I will ask the Editor of MAN to grant me the requisite space in which to deal with the problem, that of Mother Earth, which Mr. Hocart has raised.

Yours faithfully,

ERNEST JONES, M.D.

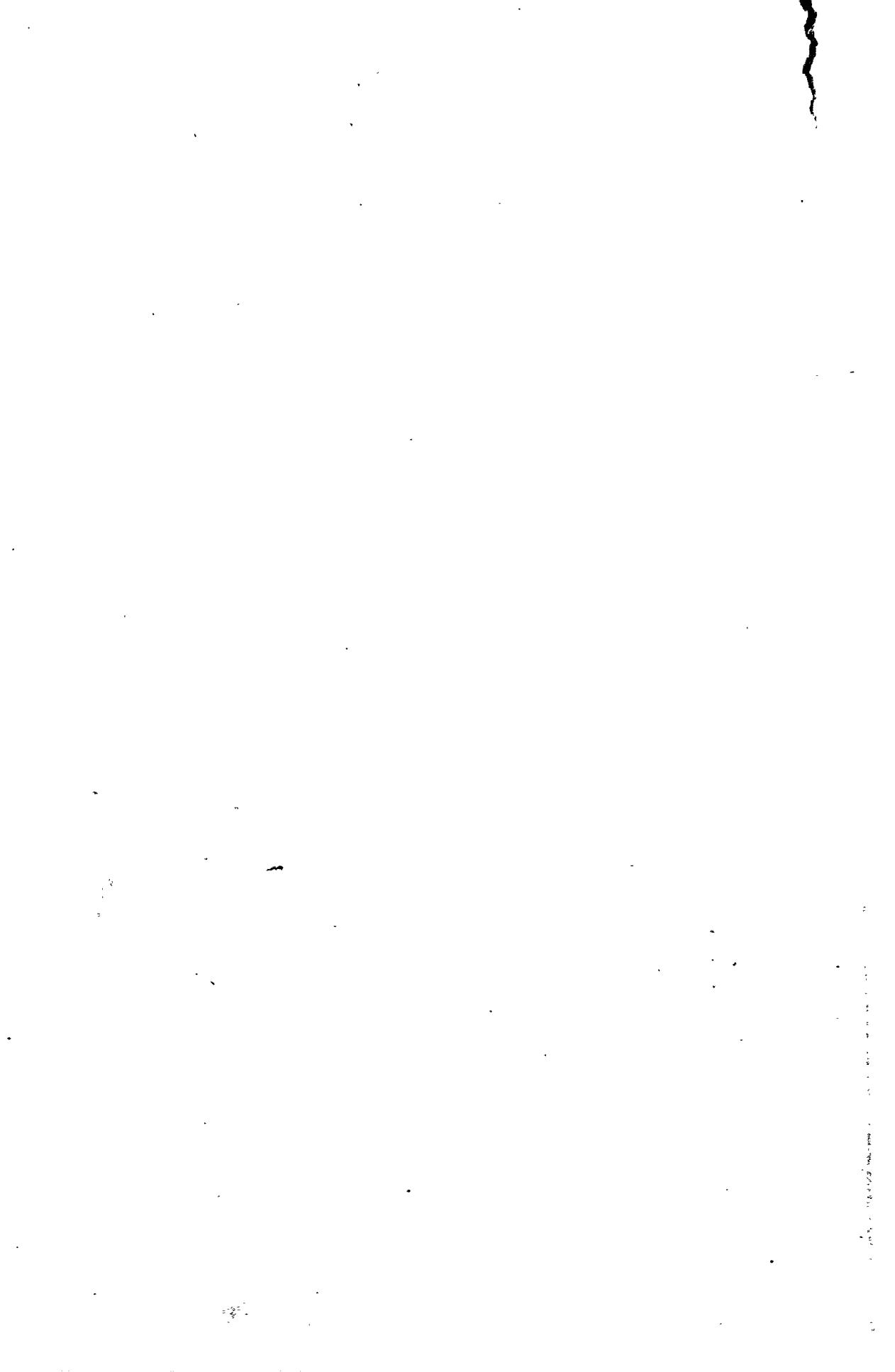
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